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ART. I.—*An Essay on the Certainty of Medicine.* By P. I. G. CABANIS, Member of the Senate, and of the National Institute, Commander of the Legion of Honor, Professor in the School of Medicine of Paris, Member of the American Philosophical Society, &c. &c. Translated from the French by R. LA ROCHE, M. D. Philadelphia. R. Desilver. Octavo, pp. 119.

M. CABANIS has long been known as a zealous and ingenious vindicator of the Medical profession. Although it is not now so fashionable to ridicule the pretensions of those who profess a knowledge of human diseases and their appropriate remedies, as it was in the last century, when the love of hypothesis afforded frequent opportunity for the witty critic to display his humour, yet we are not entirely exempt from the persecution of a certain class of men, who disregard our principles because they are not mathematically demonstrable; and of another, who think that because we cannot in every instance avert the calamities which "flesh is heir to," consider us as fair game for their humorous propensities.

It is undoubtedly desirable that arguments against the usefulness of the medical profession should be answered, and that every enemy to useful science should be overcome.

In desultory conversation it frequently happens that ingenuity in argument is more successful than sound reason and confident knowledge, in determining a contested point. This arises, not from a deficiency of zeal and confidence in him who defends the truth, but from his ignorance of those arguments which are best calculated to disarm false logic of its imposing influence, and to expose the vanity of prejudice. Every man is conscious of the existence of some truths which he cannot demonstrate,—he believes, he knows, that they exist, yet he knows not how to convince another of their existence. Hence it is that men of learning, and men of great accomplishments in science and art, have so often become the subject of the satirist, or sat to the pencil of the humorist; and hence, also, is the amiable philosopher so often obliged to shrink from the coarse and mischievous railery of profane and unfeeling ignorance.

In recommending a work purporting to be a defence of the certainty of medical science, we do not seek an opportunity to prove the usefulness of physicians to the human family; we are aware that their claim to a rank among the best benefactors of mankind, is only opposed by those who are remarkable for their ignorance, their envious, their malicious disposition, or for unaccountable prejudice. But we wish to arm every member of the medical profession with weapons for the defence of his professional principles against every attack, from what source soever it may come; and although we are conscious of enjoying the respect of all reasonable men, we do not consider it an useless task to show them that their good opinion is not a mere compliment to us, but justly our due.

Medical science is an assemblage of various species of knowledge; or, we may say, of many natural sciences, which have no connexion but in the brain of the physician. Hence great variety of talent is necessary to the acquisition of a knowledge of medicine, and for condensing from a thousand sources its heterogeneous materials, and deducting therefrom general

principles. To form a system of medical practice which will include every case of disease, it is necessary that those sciences which contribute to medicine should themselves be perfect; this is not the case with any one of them; therefore, the physician, how splendid soever may be his talents, or indefatigable his industry, is not justly liable to censure for the exceptions which may be found to all the known laws of his profession, or to his most rational theories. But while we admit that much yet remains to be done in the vast field of medical science, we also claim credit for what has been done. We will not consent to be tried by the antiquated maxim,—"*Dato morbo, invenire remedium proportionatum.*" It is not pretended that the exact nature, situation, and extent of every disease is known, nor do we pretend to a knowledge in all cases of the most appropriate remedy for diseases which are best known. But this affords no argument against the certainty of medicine, because we are seldom confined to one remedy in any form of disease; one remedy may, on various accounts, be preferable to another, but because we are not able to discriminate justly between them, shall we reject the use of either? Again, we may not possess that accurate knowledge of the character and circumstances of a disease which would enable us, by the application of an appropriate remedy, immediately to arrest its progress,—yet there are phenomena which indicate its progress or its decline, thus testing the virtue of the remedies employed.

The phenomena which indicate the existence of disorder in the system, are very numerous and very dissimilar, as the particular diseases which give rise to them vary in nature, location, and extent. But a certain set of these phenomena are frequently observed to be coincident, and if the same phenomena are exhibited in the persons of several individuals, it is reasonable to suppose that the same disease exists in every case, and experience supports us in this opinion. By means of this kind we are enabled to discriminate between diseases, to study their characters, and to apply their remedies with a

very reasonable prospect of success,—notwithstanding that an Omniscient Being might have done the work in less time.

Physicians can appeal to the history of their profession for illustrations of the vanity, as well as the pernicious consequences, of that spirit of generalization which frequently arises from a desire to reclaim their profession from the charge of irregular empiricism. During the infancy of science and literature, when learning was confined to a few, it was important to every philosopher to have a system or doctrine of his own. A *system* ought to be the result of a careful observation of fact, but when fact is deficient, imagination supplies its place. Hence, the ancient history of medicine abounds in theories which display the poetic imaginations of their authors, rather than the investigating industry which characterises the modern benefactors of science. More recent observation has induced the cultivators of medical science to shake off the trammels of inconsistent theory, and to despise the pedantry of antiquity; and devoting themselves to the investigation of truth, they have accumulated a vast fund of knowledge, capable of being applied to useful practice. Thus, instead of disbelieving the evidence opposed to favourite theories—theories are now projected only on the authority of incontrovertible fact.

It appears, then, that the medical profession does not seek respect through the influence of an imposing theory, but demonstrates at every step its practical importance in affording an asylum to millions from the persecution of disease. Our author, in treating of the certainty of medicine, arranges the objections which are usually urged against the profession, under seven heads, by which means he is enabled to answer each by fair and distinct argument.

The first class of these objections are founded on the impossibility of investigating, *ad demonstrationem*, the principle of life. The second is drawn from the difficulty of discovering the proximate cause of disease. The third arises from the susceptibility of change, incident to some diseases.

and the complications of various disorders with each other Our ignorance of the *modus operandi* of medicines form a fourth class. The fifth consists of the uncertainty of the results of physiological and pathological experiments. The sixth regards the mutability of medical theories, and the frequency with which a new doctrine has been built on the ruins of a theory once considered as established. The seventh class of arguments against medical certainty, is founded on the difference of opinion which obtains among physicians themselves, and the consequent variety of practice that is observable among them.

Our limits do not admit of a detail of the ingenious and reasonable manner in which M. Cabanis has refuted the arguments of the enemies of medical art, on the above mentioned grounds. Nor, perhaps, would it be proper to give any more than an abstract of the most remarkable, as every intelligent physician who reflects on this subject, can draw from the stores of his own experience, a sufficient mass of evidence in support of all the positions which our author has taken.

It appears to be an unanswerable argument, that a profession, having for its object the protection of life against what are called morbific agents, must necessarily remain in a state of imperfection, and its practitioners must consequently be without confidence in their own prescriptions, while we are ignorant of the precise nature of Life,—whether it be a principle *sui generis*, an immediate cause of vital phenomena, or an effect of certain forms of matter, acting and re-acting upon each other, how matter becomes endued with life, and what circumstances are necessary to the connexion of this principle with matter. This is undoubtedly an interesting subject, to the investigation of which many have been prompted by curiosity, and also by the laudable desire of extending the boundaries of medical knowledge. But however important the knowledge of ultimate principles may appear to be, and however specious the arguments in favour of its necessity,

we must insist, that it would add but little to the utility of medical practice.

It is wisely ordained, that the various parts of the animal system are endued with a sensibility by which morbid action, or the presence of stimuli, capable of deranging the natural functions, are detected. Although we cannot demonstrate the cause of life, or the immediate cause of death, yet we are enabled, through the indication of a variety of symptoms, to discover the location of morbid action, and the nature of this action, sufficiently to guide us in the application of a remedy. Anatomy has discovered to us the structure of the human body, and shown us the organs by which its functions are accomplished, and also the connexion between various organs, by which pain and other indications of disease in one, is referred to actual disease in another. Hence a great deal has been done towards establishing a correct diagnosis, in which consists almost all the difficulties of the medical art. It is true, that our want of correct diagnosis, in a few instances, affords foundation to a few *opprobria medicorum;* but how are these to be removed? not by searching for the essence of life, which, when known, would in all probability throw little or no light on the nature of the disease, nor, indeed, even assist in discovering its location.

Strict attention to the symptoms, the change which they undergo through the influence of certain medicines, with the age, habits, and constitution of the patient, and *post mortem* examinations, have done more towards the detection of error, and the improvement of medical science, than all the dreams of philosophy and vain reasoning upon ultimate and general principles. Let us not be charged with encouraging empiricism; we wish not to explode all systems and theories comprehending general principles of tried and acknowledged utility.—But we wish merely to insist that all doctrines, to be useful, must be drawn from the observance of fact. It is useless to erect a theory on any other foundation, nay, it is even in some instances pernicious, inasmuch as its erroneous im-

pressions oppose the establishment of truth, and it is more difficult to wean the mind from a favourite delusion, than to direct it successfully to the investigation of truth.

Many speculations have been entertained respecting the principle of vitality. The world has some time been amused with the contemplation of the vibration of atoms, and at another with an ethereal spirit. Sometimes life was a cause, having power over matter, and again, a mere effect of material combinations. But skill in these speculations is of no value at the bed-side of the patient,—vastly different is the path which will conduct the physician to a knowledge of remedies, and give him power over disease.

When we observe that certain symptoms occur at the same time, and that in every instance of the appearance of these particular symptoms, one and the same part has been affected with disease, and when we find also that different forms of disease in the same part, are invariably indicated by a corresponding difference of symptoms, we must acknowledge the agency of some general principle, which rules the animal economy by fixed and unchangeable laws. It is not necessary that we should be able to contemplate this principle in an insulated manner, free from its connexion with the phenomena of the organic system. Medical certainty only requires that such a principle should exist, and that the laws of the animal economy should be certain in their effects;—in other words, that the maxim, *similar causes under similar circumstances, will produce similar effects*, should be exemplified in the animated machine, as well as in the subjects of physical science. That this is really the case, has not only been long established by experience, but is daily demonstrated in the person of every living animal. These laws we discover in their effects, and we become acquainted with the existence of a certain disease in a certain organ, by observing a certain symptom or class of symptoms, which are known invariably to attend such disease.

Our knowledge of remedies is derived from the same

source, namely, observation and experience. We do not attempt to *demonstrate* that jalap will produce catharsis, tartrate of antimony emesis, or that certain applications will allay the violence and pain of inflammation,—nor could the most laborious and ingenious course of philosophising even lead us to such conclusions.—But what would we think of the intellects of a man who, on this account, would object to the use of these articles, when the certainty of their effects has been attested by invariable experience?

"Let us be judged by our professions," and it will then be found that the physician is not obnoxious to the charge of arrogance, though he may claim the possession of more knowledge than is usually acquired by other men. The medical profession is rich in practical rules, whose infallibility has been so established by experience, as to enable the physician to practise, not only with promptitude, but with certainty of success. If, then, experience is a sufficient foundation for practical rules, should the possibility of deriving rules from another source argue imperfection or uncertainty in medicine?

It may, finally, be argued, that there are some forms of disease that have hitherto baffled every attempt of the healing art. The greatest number, if not all such cases, when candidly considered, are harmless to the pretensions of medical science,—for they may be found to be in their natures incurable, to have arisen from original defects of the constitution of the patient, or to have, almost at the very commencement of their career, committed some irreparable violence to the system. Thus, we cannot charge to medical uncertainty the death of one whose lungs have been destroyed by ulceration; of another, whose arteries have become ossified; or of a third, who has suffered from the rupture of a cerebral artery; nor can we see how a knowledge of the vital principle could enable us to extend the aid of the materia medica to such cases.

With regard to the second objection, it is true that we have not so far gratified the refined taste of modern philosophy, as

to have traced every thing connected with the medical art to first causes.—But at the same time we insist that this is not a fair test of the merit of any art. We are all sensible of the advantages which we derive from the practice of various arts, the manipulations of these are said to be predicated upon certain principles;—if we examine these principles we find them to depend upon other principles too recondite to admit of demonstration;—thus, whatsoever path we take towards the discovery of a primary cause, we will find, that although reason may convince us of its existence, human ingenuity is incompetent to the task of its development. Let us grant that every art is conducted upon empirical principles, and that we make practical deductions from natural phenomena which are inexplicable, shall we therefore despise the benefits derived from such arts? Shall we refuse to eat bread because the baker cannot trace the phenomena of fermentation to a first cause? We know that the frequent introduction of digestible matter into the stomach is essential to the continuance of life, and we know that bread is digestible food, and we are ready to call him a madman who would object to its common use, by any train of argument founded upon our ignorance of the causes of its existence, the apparatus by which it is assimilated to the animal system, or of the laws by which the digestive apparatus is governed in effecting such assimilation.

So, also, in the art of medicine, we know, by long and unexceptionable experience, that certain substances may be necessarily applied to the human system, for the removal of pain and other symptoms indicative of disorder. Ample observation warrants the belief, that without the interference of such remedies, these symptoms of disorder would be the precursors of inevitable death. Is it then reasonable that we should refuse the remedy whose practical utility is established, because we may be ignorant of the abstract principle upon which it operates? Nor would a knowledge of the *modus operandi* of medicines render their operation more certain or satisfactory. We can avail ourselves of an effect of which we

know only the immediate cause, with as much certainty as if we had discovered the proximate cause to be the effect of one more remote.

The third objection is as easily refuted. A proximate cause of disease may, from time to time, vary in intensity, change its location in the system, involve in morbid action a lesser or a greater number of parts, exhibit features somewhat different under the various circumstances of constitution, habits, climate, &c. and perhaps become complicated with other causes. Nevertheless, there is always a certain train of symptoms sufficiently distinct to guide the attentive practitioner in his general prescription; and the cases are rare indeed in which, if from causes beyond his controul, he cannot avert the stroke of death, he cannot, at least, relieve his patient from much of the anguish of unrestrained disease.

It not unfrequently happens that this very complication of disorders, supposed to render the art of medicine too intricate for the scope of human ingenuity, points out a remote cause of the general disorder, and thus enables the physician, by striking at the source, to remove all the branches of diseased association. How often do we find the cough, threatening the horrors of consumption, the indigestion, nausea, and pain of a debilitated stomach, and the consequent head-ache disappearing before the medicine which has power over a disordered liver!

With regard to the fourth objection, it is of little or no importance for us to know the *modus operandi* of medicines. While we know that certain causes will produce certain desired effects, and that these causes are so completely at our command that we can at pleasure avail ourselves of their effects, and thereby accomplish our final object, surely it is mere curiosity that would prompt us to the investigation of the manner in which the effect is connected with the cause. If we wish to evacuate the alimentary tube, or to promote the action of the superficial exhalents, we know the substances whose exhibition will effect our object, and the result is just as satisfactory as

if we had followed them into the dark recesses of the animal machine, and witnessed their occult operations.

The fifth objection, we contend, is not well founded in fact. A great variety of experiments have been instituted for the purposes of ascertaining the nature and seat of various diseases, and of throwing some light on the *modus operandi* of medicines. It is true that many attempts of this kind for the development of truth, have not been followed by satisfactory results. Nevertheless, many of the labours of the experimentalist and the dissections of the pathologist have extended our knowledge of the laws of the animal economy, pointed out the peculiar features of certain diseases, and finally enabled us to prescribe with confidence, and save the lives of thousands. No single experiment, perhaps, is sufficient for the establishment of a law, however plausible its *rationale* may be and although it were admitted that no law, in any of the natural sciences, is perfectly established, yet there are many so rationally deduced from experience, that we are enabled to adopt them advantageously as rules of practice; many such rules, whose validity may not be absolutely demonstrable, have been applied to practice with such beneficial results as to place their utility beyond the reach of objections founded upon a few trivial exceptions.

By means of experiments the resources of the healing art have been much extended. All sciences and all arts are the result either of the accidental observance of natural phenomena, or of experiments instituted for the elucidation of preconceived ideas. Many of these have been ingeniously contrived and industriously conducted, with a view to certain results which have not been obtained. But although the desired object may not be attained, yet other discoveries are made, which prove to be of greater importance. Thus we trace the origin of the great science of chemistry to the vain attempts of the Alchymists for the transmutation of metals and the composition of an universal panacea.

But the labours of physiologists are not always unsuccessful. Not only has our knowledge of the nature of disease been much improved by them, but our *materia medica* has been greatly enlarged and corrected since it has become fashionable to pursue the experimental plan, and to seek causes through the contemplation of facts.

The next argument, according to our author's arrangement, against the certainty of medicine, viz. that "if the science of medicine rested on a solid foundation, its theory would at all times have been the same, and its practice more particularly would not have undergone, as it has, from one age to another, the most extraordinary changes," instead of exciting disrespectful feelings towards the science of medicine, ought to reflect the greatest honor upon the intelligence and industry of its cultivators.

Every science and every art has had an origin. With regard to each, a few observations gave rise to a few maxims, and constituted an imperfect system, susceptible of improvement, and perhaps liable to essential change, as in the progress of investigation, new facts came to light, requiring more comprehensive theories for their arrangement. Medicine, whether considered as a science or an art, has also had its birth and infancy. As a science, it is, and must always be, peculiarly susceptible of improvement. Its principles, as we have already remarked, rest upon the principles of a number of distinct sciences, none of which have yet arrived at perfection. Even mathematical science, one of the first that was cultivated, and one whose rules are more perfectly demonstrable than those of any other, is yet in a state of progressive improvement. But medicine is not a demonstrable science, it more particularly depends upon observation of insulated facts, and without being susceptible of being comprehended in any general theory, its practice is regulated by many maxims, independent of each other, yet resting on the best foundations, viz. fact and experience. Yet we may be asked why, if our science possesses some fixed principles, do

physicians practise differently in similar cases? This is undoubtedly the most popular as well as the most imposing objection that is urged against our scientific pretensions.—But it is easily answered.

In the first place, with regard to the difference which obtains between the practice of the ancient and the modern physicians, it is sufficient to mention the progressive improvement that medical science has undergone; in the course of which, new facts have overturned old theories, and increasing knowledge in this, as well as in other sciences, has removed deep-rooted prejudices. New doctrines and more enlarged ideas, are the natural result of successful investigation of fact, therefore the novelties of modern practice are a natural result, and as such are exempt from animadversion. Secondly, that cotemporary physicians should differ in many particulars in their treatment of certain diseases, may arise from the unimportant nature of the matter in question. But their differences frequently involve a general question, apparently of great importance, insomuch that two physicians will treat the same disease according to different systems of practice. It is true that such is often the case, but in such instances the disease admits of various methods of treatment, and each method may employ a great variety of medicinal substances to the same purpose. There are but few, very few, diseases that are conquerable only by one particular remedy;—the generality of them are vulnerable at many points, they will yield to any one of a variety of remedies. Hence the difference which the unlearned think that they detect among physicians, is so far from casting disgrace on the profession, that if all physicians should treat a given disease in one and the same manner, it might be regarded as one of the most remarkable of fortuitous coincidences. Many of the differences between cotemporary practitioners also arise from the circumstance of the continual improvement of medical science. The old practitioner, who has long since lost the enthusiasm which stimulates the student to industrious research and to

an intimacy with the latest improvements in each branch of his profession, applies in practice those principles which he learned in his youth, and he differs from a practitioner of more modern date, either from his ignorance of recent discoveries, or from prejudice in favour of early impressions. Hence the folly of ascribing wisdom to age, and skill to decrepitude.

But if individuals are to be found who do not perfectly understand, or properly apply the principles of their profession, still this is no argument against the profession itself, either as regards its utility, or the stability of its principles.*

Before we dismiss this part of the subject, we must consider another and a very fruitful source of animadversion upon our profession. It has been remarked above, that diseases were liable to be complicated with each other; or, in other

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* With regard to this part of the subject, it may not be improper, nor perhaps, altogether useless to remark, that the medical profession is frequently charged with the faults of those who are in no way connected with it. In every part of the world ignorant pretenders to knowledge in the healing art, may be found, and there are but too many credulous people willing to lend them their support. The consequence is inevitable.—Disappointed in the relief which they expected to derive from medical art, the dupes of quackery vent their anathemas against a science which they never employed, instead of ascribing their misfortunes to an undue faith in newspaper advertisements and the use of nostrums, having no claim upon their confidence.

It is too frequently the case, particularly in the southern and western sections of the United States, that the character of our great and noble profession is in the guardianship of ignorance and prejudice. The empiric who boasts of certain secret remedies, unknown in the universities, or too valuable for gratuitous publicity, will frequently acquire both fame and fortune, notwithstanding his innumerable and often, fatal errors. The failures of such, and of others not less remarkable for an impudent substitution of a certificate of membership of a medical society, for a diploma, ought not to be brought in account against the certainty or the utility of medicine.—Yet to the shame of the community, that submits to such imposition, these form the most fruitful source from whence the lovers of detraction draw their vexatious arguments against professional merit.

words, that various causes of morbid action may exert their influence upon the human system at the same time.* Hence various shades of disease arise, which indicate as great a variety in the application of remedy. These things can be seen by the experienced and judicious practitioner, and he compounds his medicines accordingly. But in every case he is expected to give a name to the disease; he does so for the gratification of his patient, well knowing the uselessness of medical nosology, and aware of the imperfections to which it must always be liable. Yet if he does not prescribe for this *name* as another practitioner would do, in a case so dissimilar as to call for dissimilar practice, a disgraceful reflection is cast upon the profession by those whose ignorance renders them totally incapable of forming the least degree of judgment in the case.

M. Cabanis has placed many of the cavils against his profession in their proper light; and by exposing the folly in which they originated, he has shielded "the great science" from the profane attacks of its unprincipled enemies; and some enemies it will probably always have, for the successful cultivators of this science are justly considered as a superior order of spirits, whose acquirements are beyond the reach of the common order of men, and it is but natural, that perfection, if it exists on earth, should be sought for here. But, alas! so little are men disposed to be satisfied, that leaving respectable mediocrity out of the question, the most extravagant poets can invent nothing sufficiently devilish for the depravity of their taste, nor, on the other hand, can the most finished models of human virtue, talent, and industry, satisfy their unreasonable craving after ideal perfection.

Much credit is due to Dr. La Roche for his correct trans-

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* We have chosen this form of expression for the sake of brevity;—we include in the term morbific agents, unfavourable states of the atmosphere, improper food, pernicious habits, and every thing that can affect the health of the system.

lation of this excellent work, by means of which it may be much more extensively useful in this country, than it could be in its original language. H.

ART. II.—*An Essay Concerning Tussis Culvulsiva, or Whooping-cough, with Observations on the Diseases of Children.* By BENJAMIN WATERHOUSE, M. D. Boston, 1822.

WHATEVER merits we may feel disposed to recognise in the present essay, it will certainly appear evident to all those who have already perused its contents, that the credit of offering a methodical arrangement, and clear exposition of the various subjects of which it is composed, cannot with propriety be conceded to its author. In order, as he avows, to "enliven a dry subject," Dr. Waterhouse has thought fit to incorporate among his observations on the whooping-cough, long and dry anatomical descriptions, and very many physiological and pathological disquisitions, as applicable "to asthma, dyspepsia, or consumption," and he might have added to many other diseases, as to the one forming the main object of his inquiries, and which, although they may have served admirably well, the purpose of swelling the volume to the respectable size of one hundred and fifty-two pages, are nevertheless, in our opinion, altogether useless, and calculated only to render the reading of it fatiguing and painful, and a review of its contents still more so.

Dr. Waterhouse sets out with the declaration, that he was led to commit his thoughts of whooping-cough to paper, from observing the difference in opinion among physicians of the nature, causes, and treatment of this painful distemper. From this we are naturally led to conclude that his intention is to throw, if possible, some light on this obscure subject, an intention which recommends him to our warmest praises; but whether or not he has succeeded in the attempt, and whether we shall know more on this disease *after*, than we did *before*,

reading the present volume, are questions we shall next endeavour to determine.

In respect to the history of whooping-cough, our author is inclined to believe, that it was first described in the seventeenth century, and says, that the anxious student could see no traces of it through the dark ages, "that he could learn nothing of it from the books of the Arabians, and that even after the resurrection of letters in Italy, down to the art of printing, he could not discover that the distemper had ever existed." "It is strange," he continues, "terrible and destructive as this disorder is, that we have no description of it in any book prior to Dr. Thomas Wallis, who taught at the university of Oxford, and practised in London between the years 1650 and 1680; of course, cotemporary, several years, with Sydenham." In a review of this nature, it is not, perhaps, necessary to dwell with much minuteness on the precise moment when a disease has been first described; nor is it our intention to do so. Nevertheless, we cannot refrain from remarking, *en passant*, that our author, in confidently refusing the credit of first describing Pertussis to Wallis, has laboured under a small mistake. In all attempts to establish with precision the date of particular events, the strictest attention should be observed,—and the minutest investigation should not be neglected. Had Dr. Waterhouse been attentive to this wholesome precept, and extended his researches into works of an older date than those he has consulted; or had he ever opened Sprengl's History of Medicine, he would without difficulty have found that Mezeray, in his History of France, describes the disease as having prevailed epidemically in France, in the years 1414 and 1510;—that Coyttarus and Pasquier mention it as having appeared again in 1557, and that it again made considerable ravages in Europe in 1580. But we cannot too strongly insist on the little necessity of examining when and where a disease, such as the whooping-cough, has first made its appearance, and been noticed by medical writers. Consistently with this opinion, therefore,

we hold the conjectures of Nil de Rosenstein, respecting its African or East India origin, or his endeavours, agreeably to the fashion of his time, to discover some passages that have an obscure reference to it in the writings of Hippocrates, to be of no value to science or humanity. A study of books will assuredly teach us when, and by whom, it was first described, but never when it for the first time appeared;—since all the causes of the disease having in all probability existed from the creation of the world, the malady itself, though not described, must necessarily have afflicted its earliest inhabitants. Such indeed *must* be our opinion, unless we adopt the belief of Dr. Husson, who, in his work on vaccination, advocates the hypothesis of the identity of whooping-cough with the distemper of dogs. If this opinion be correct, and if it be established that the distemper of dogs is not the product of the same causes as whooping-cough, then we might perhaps believe that, in 1414, by some accident or other, the disease was communicated, for the first time, from a dog to the human species.—We marvel that Dr. W. has not thought of this hypothesis.

But having dwelt sufficiently long on Dr. Waterhouse's Prolegomina, we proceed to notice the body of the work; the first chapter of which is appropriated to an enumeration of the many names which at various periods, and by different authors, have been employed to denote the whooping-cough—to their various etymologies; and also to a lengthened description of the malady. "Dr. Robert Watt, of the city of Glasgow," Dr. Waterhouse says, "who has written industriously on this distemper, tells us that in North Britain, it is conspicuously called whooping-cough, and *kink-cough*, and hence the derivation chin-cough. This Scotch provincialism, kink, is, it seems, synonymous with fit, or paroxysm; for they say in Scotland, in common parlance, "a *kink* of laughing,"—"a *kink* of crying,"—and "a *kink* of coughing." The phrase is a novelty in our country. Dr. Johnson suspects that the term *chin* cough may have come from

the Dutch word *kinckin*, to pant, or breathe short,—yet we doubt the whole of it. "Now chin-cough is a corruption of the Dutch, or rather Teutonic word kindhoest, and means simply child's-cough, from *kind*, a child, and *hoest*, cough; and is precisely synonymous with the Latin term *Tussis Puerilis*. It was necessary to say thus much, because many people have an idea that *chin* cough, is a milder species of whooping-cough."

In respect to the symptomatology of whooping-cough, Dr. W. not content with presenting an original and distressingly minute description of the various forms assumed by the disease, has introduced Dr. Wallis's, both in Latin and in English. What the advantages are which may arise from this accumulation of description, other than the one we already hinted at, we are at a loss to determine. The detail of symptoms, which he has himself given, was sufficiently long and precise, to supercede the necessity of adding a very imperfect one, extracted from a work familiar to many. Be this, however, as it may, we feel little disposed to transcribe here the six or seven pages which the author has devoted to this part of his subject, as whooping-cough is already well known to our readers—and because, were this not the case, faithful descriptions of it may be found by a reference to any of the elementary books of medicine, and more especially to Dr. Dreyssig's excellent work on medical diagnostics.

On the subject of the different stages of the disease, Dr. W. observes, "it is customary of late, with most medical writers, to divide every acute disease into three distinct stages. We are not certain that we can draw the line with due precision in the tussis convulsiva. It appears to us, however, that the characteristic symptom, the *whoop*, is formed between the fifteenth and twentieth day; and that a change or crisis appears about the fortieth; but when it continues longer than the fiftieth, it becomes chronic; and then it may run on to six months and more, with occasional remissions. During this indisposition, the weakest organ or function suffers; or

that disorder to which the constitution is most liable, aggravates the original malady." He further remarks, that he should have remained ignorant of some particular symptoms of the whooping-cough, had he not recently suffered under it, and compared his symptoms with those of other adult patients, qualified for relating their sensations correctly. From them he also learned that, during the early paroxysm, the urinary organs, from the kidneys downward, felt unusually sensitive; while the stomach had now and then a glow, accompanied with a transient nausea, occasional *horripilatio*, and some other signs of a disordered system tending to fever.

The succeeding chapter of the work before us, contains an inquiry into the seat of the whooping-cough, together with observations on the mucous membranes. On the seat and nature of this affection, much diversity of opinion has prevailed among medical writers. Whilst by Willis, Hoffman, Fothergil and Cullen, the disease was referred purely to a spasm; by others, among whom was Sydenham, it is regarded as a disease of high inflammatory character; and, on the contrary, by Brown, as one of asthenic action, requiring the Sthenic treatment. Some physicians have placed its seat in the stomach. Among those who advance this opinion, Pinel stands conspicuous. Referring the primary link of the disease to that viscus, he regards the irritation of the lungs as the effect of a purely sympathetic action; nevertheless, by one of those inconsistencies with which his work, in common with all other nosological arrangements, abounds, he classes whooping-cough among the *neuroses* of the respiratory organs. Buttu considers the disease to depend on a deranged state of the primæ viæ.

"It is not very surprising," says Dr. Waterhouse, "that physicians should differ in opinion as to the nature, and the cause of whooping-cough; but it must appear strange to the ordinary reader, that medical men are not yet agreed as to the *seat* of it." In order, therefore, to clear up, in some measure, the almost impenetrable obscurity in which the subject

is involved, he sets out by asking, under what head in nosology, we are to locate this disease? And, in the next place, endeavours to decide this *important* question. We fear, however, we shall have little reason to congratulate him on his success; or be satisfied with our own,—in placing before our readers a clear summary of his opinions, owing to the great confusion existing in the selection, as well as in the arrangement of the materials composing this chapter. He commences his subject by endeavouring to prove that *post-mortem* examinations can throw but little light on the nature of this and other diseases; and that consequently Dr. Watt in asserting that, having had recourse to dissections, he had derived from these the most solid satisfaction, since they pointed out the nature of the whooping-cough at once, and settled every doubt in his mind respecting the mode of treatment which ought to be pursued, had advanced more than could be satisfactorily proved. "I value *necrotomy*, he says, but have less confidence in *post mortem* appearances, than in years past. All we can see in the dead body is redness, swelling, ulceration, and fracture. Besides, seeing, as well as hearing, and pulse feeling, have their grades of perfection, depending on cultivation."—"The cold, stiff, dead, collapsed, and half putrified cadaver is so different, so widely different, from the warm, turgid, panting, pulsating, breathing, and all moving, living body, that it requires no small experience, nay, it requires often consummate judgment to determine accurately the cause of symptoms, and of death. We are ready to grant that the nature, and even the seat of many diseases, have not, hitherto, been determined satisfactorily by *post-mortem* examinations; and agree with our author, that hydrophobia, some cases of tetanic affections, of epilepsy, and, indeed, of the whole class of spasmodic disorders, leave so few traces behind them, that we cannot derive from the minutest dissection a very perfect knowledge of their nature. But this does not do away the utility of dissection, or refute the cor-

tness of the observations made by Watt and others, respect-

ing the seat of whooping-cough. But let us see to what results Dr. Waterhouse arrives on this latter subject. "If," says he, "we cannot fix precisely on the muscular part which is primarily affected in this distemper, there can be no doubt as to the *membranaceous* part. We are certain that its seat is in that delicate and very irritable *internal skin*, denominated the *mucous membrane*, which lines all the passages from the eyes, nose, ears, mouth and throat, downwards, including all the emunctories, outlets, and inlets of the body." After this declaration, our author indulges in anatomical and physiological researches on the mucous tissue, into which, however, we shall not follow him; and he next endeavours to ascertain the portion of the membrane primarily affected in whooping-cough, and as his notions on this particular subject, if not original, are at least explained in a manner peculiar to himself, we shall transcribe them in his own words. "It is presumed," he says, "that on this mucous membrane thus constituted, endowed and governed by the general laws of life, impressed on the whole system, *the specific contagion of whooping-cough first seizes and operates.* It does not appear that it affects the whole mucous membrane of the head and breathing organs at once, as in catarrh; but it seems at first to occupy a small part only, and thence to creep on by degrees. The parts first attacked, appear to be extra-thoracic; and we have reason for believing that they are the portion of the mucous membrane, which covers the upper parts of the alimentary canal, including the pharynx, œsophagus, and that entrance of the stomach called the cardia; when the mucous membrane is observed to be disposed into longitudinal folds, conformable to its power of dilatation. It thence extends to the diaphragm one way, and to the larynx, trachea, and air-vessels of the lungs, in extreme cases, another; but in most instances, the lungs are affected by consent or sympathy; and not organically, until the distemper has changed into a chronic disease. Even then we can hardly allow it to be bronchitis, whose origin is catarrh, but a state or condition of the respi-

ratory apparatus, that wants a name; and is more like the chronic state of measles, than any other."

Passing over Dr. Waterhouse's long remarks, and the whole train of reasoning by which he endeavours to prove that there exists an essential difference between whooping-cough and catarrh in croup, we proceed to notice his third chapter, in which he resumes the investigation of the mucous membranes, and adds a disquisition on the diaphragm. "From what has been said," he observes, "it appears highly probable that whooping-cough has not the same cause, nor does it, in the first instance, occupy the same parts of the mucous membrane, with *catarrh*, *croup*, or *bronchitis*. It appears, also, that the *Systema Spiritale Pneumonicum*, is not primarily, but secondarily, or sympathetically affected in whooping-cough.

"Where then is the primary seat of this terrible distemper? The more I contemplate the human frame, while in health, and the more I scrutinize the same body disordered, the more I am confirmed in the opinion, that if to the *stomach*, and *duodenum*, is added the *diaphragm*, we shall find that *they are the original or primary seat of nearly all the internal distempers that flesh is heir to.*" It appears pretty evident, from this exposition of our author's theory, respecting the seat of whooping-cough, that he has not offered any thing original; since it has already been shown that Pinel, among many others, placed the primary link of the disease in the stomach, and regarded the affection of the respiratory organs as sympathetic. In many respects, the reasoning of Dr. W. is erroneous, and he frequently involves himself in assertions that are at variance with correct anatomy, and the true principles of physiology. His notions concerning the extension of the disease from the diaphragm, will serve to prove this. In many parts of his work he admits, in this disease, the existence of spasm, which he well knows exists only in the muscular fibre; but here, neglecting to refer the affection of the diaphragm to a spasmodic condition of its fibres, he says, that the disease is seated in the mucous mem-

brane of the stomach, and duodenum, and "thence extends to the diaphragm, and to the larynx, trachea, and air-vessels of the lungs," from which one unacquainted with anatomy would naturally conclude that the diaphragm is lined with a *mucous* membrane.

For our part, the more we see of whooping-cough, and reflect on its nature, the more we are disposed to coincide with Darwin, Watt, and the disciples of the physiological school of France, in regarding it as consisting of an irritation or inflammation of the mucous membrane of the respiratory apparatus, extending itself, sometimes to the parenchymatous substance of the lungs. Nor do we hesitate to state it as our decided opinion, that in many respects it differs from bronchitis only in the spasmodic affection of the muscular fibres of the diaphragm and other parts, which supervenes after a few days, and sometimes weeks continuance of the disease, and that the vomiting and other gastric and intestinal derangements are the effects only of the spasm already noticed, or of accidental complications, which are likely to occur in every affection of the chest. In refuting the opinion of Pinel on this subject, Broussais remarks, "we cannot doubt that a gastric irritation is capable of exciting cough; examples of it are daily met with; but it is only a special *exaltation* of the sensibility of the mucous tissue of the lungs that can determine the convulsive cough of Pertussis. This *exaltation* itself depends on a mode of inflammation, equally peculiar, either in its cause or degree, of the mucous lining of the trachea, or bronchæ; but this species of catarrh passes very easily beyond the limits of the irritation producing the cough, in order to change itself into a very intense inflammation of the pulmonary apparatus, and to complicate itself with gastro-enteritis." We are well aware that a modification of this theory of irritation, has been proposed, and has received the approbation of some names of no little respectability in the profession; but we must confess that it does appear to receive support from the established principles of physiological

pathology. It is maintained, for example, that the irritation which constitutes the disease is purely nervous, and bears no analogy to common irritation. This difference between the nervous and vascular irritations we have never been able to discover, since the former invariably precedes the latter for a longer or shorter time, and the latter runs into the former;—this is well exemplified in the disease which forms the subject of our present inquiries; for, in every instance which is denominated the *nervous* irritation of whooping-cough, the disease assumes all the characteristic features of a true vascular irritation;—in other words, of a violent inflammation. That an analogy exists between this disease and bronchitis, is evident from the circumstance that, like the latter affection, the whooping-cough appears under the same modification of atmosphere, reigns epidemically together with it, cannot be distinguished from it in the early stages, even by the most experienced practitioners, and resembles catarrh; and when chronic, terminates in phthisis, with or without tubercles.

Postponing our inquiries into the contagious character of the whooping-cough, we return to Dr. Waterhouse. He seems to lay a considerable stress on the immense influence of the phrenic or precordial regions over the rest of the economy, and indulges in many remarks with the view of proving, that long experience, particular experiments, and our own feelings, demonstrate that there is no part of our system so liable or sensible to impressions. On the subject of this phrenic centre, in which Van Helmont had located his great Archeus, and which Buffon, and many others, regarded as the seat of the soul, our author has repeated, though in a language far less perspicuous, the ideas which the celebrated Borden and Lacaze had long ago offered in the "Idea de l'Homme Physique et Moral," and in the "Specimen ex novo Medicinæ Conspectu." After much discourse on this important centre, which he qualifies with ingenious titles of "Head quarters of the animal economy;"—"The focus of our best, as well as our worst feelings," he remarks, that the structure of the dia-

phragm would render it probable that it is an organ destined to more important offices than that of a mere partition; or *floor for the heart* to lie on, or the *leather of the bellows*, to follow mechanically the impetus of the air, or the living actions of the lungs and *ribs.*" And after a very minute anatomical description of this organ, and of its nerves, he concludes that it is *very probable that the centre of this organ, where the aorta and vena cava pass from above downwards, and from beneath upwards*, is the seat, source, receptacle, or *citadel* of that nervous energy which is suitable to the organic life and action of all the abdominal apparatus, and on which they depend for support;" consequently the seat of the phrenic region.

The succeeding chapter, on epidemics, is probably one among those the author has introduced, for the laudable purpose of "enlivening a dry subject," and we should certainly have stopped to notice some of the many remarks he has made on the subject, had they appeared to us to have possessed as much of that enlivening virtue as he would seem to attribute to them. We shall not, therefore, draw the attention of our readers from the subject more particularly under consideration, for the purpose of amusing them with speculations relative to the coincidence of comets, and volcanic eruptions, with pestilence, meteors in the atmosphere, with sickly seasons; the sickness and death of quadrupeds, fish, and insects, with wide wasting epidemics. "We have not been able," he says, "to trace the whooping-cough to any local cause, nor to any known peculiarity in the seasons. It appears in all places, in every season; and in all sorts of "*constitutions*" of the year; in the open country, as well as in the confined city. It does not appear to seize more readily any particular constitution, or habit of body."

From the foregoing observation it would seem evident, that Dr. Waterhouse entirely rejects atmospherical agency in the production of whooping-cough, and that, faithful to an opinion

he has already advanced, he regards the disease as arising solely from specific contagion.

But if it be denied by some, that the whooping-cough is a disease produced by the same general causes, as catarrh, pleurisies, peripneumonies, and is void of contagion, it must be conceded, that in some years, and in some seasons, it prevails more exsensively than at others, and even assumes an epidemical character, diffusing itself over a large portion of country; —attacking a great number of individuals, of all ages and conditions, and proving, at some times, more fatal than at others. In 1580, this malady proved fatal to 9000 children in Rome; and, according to Nil de Rosenstein, it was fatal in Sweden to 43392 children, from the year 1749 to 1764 inclusively, or 2712 in each year. Dr. Waterhouse himself (p. 17—18) has afforded us proofs, that the disease occasionally assumes the characteristic features of an epidemic; for, borrowing from Watt's treatise, he tells us that, in the fifteen years prior to 1717, only 63 were registered in London as having died of chin-cough, and 56 of cough, and that in the fifteen years ending with 1776, they amounted in the same city to 4252;— differences that could depend on no other cause than a difference in the atmospherical constitution of these different periods. Dr. Waterhouse, in reasoning on the passages we have just quoted, makes the following question: "Are we to infer from these facts, that the malady is in itself absolutely incurable, or that it is always a slight distemper?" And next answers, "neither one nor the other. We ought, by no means to conclude, that every disorder which we cannot arrest and hinder from running on a tedious length, is in nature unconquerable." From this latter remark we had naturally concluded that Dr. W., like Huffeland, Werlhof, and many other ontologists, entertained the belief that pertussis must run its course, and cannot be arrested in its progress; when, on turning to the very last page of his essay, we found the following passage, which exonerates him, if not from the charge of inconsistency, at least from that of a belief in that erroneous

doctrine. "When we are distempered and the body acting wrong, we may arrest the particular disorder by depriving it of the power of doing mischief in that particular way, by means of copious and repeated bleeding. It (the whooping-cough) may be arrested in a few days by repeated emetics and purgatives, by mercury, alone or combined with antimony."

Without enlarging more on this subject, we proceed to lay before our readers another passage from the work under review, in order to show the author's opinion of the nature of the whooping-cough. In conformity to the opinion he entertained respecting the nature and origin of all diseases, Linnæus taught, that the present malady was the product of a distinct species of insects, which, being inhaled, found a retreat in the bronchial vessels. Now, as eruptive diseases have been referred by some pathologists, even of the present day, to the same cause, Dr. Waterhouse's notion will not be found much at variance with those of Linnæus. We shall quote his own language. "If we know nothing of the remote cause of whooping-cough," he says, "we know but little more of its nature. Experience teaches us that it attacks a person but once in life. From every view we are inclined to believe it an inflammation *sui generis*, and strongly suspect that it is of the exanthematous class; occupying, at first, a small portion of the membrane." Professor Cullen, after saying that whooping-cough seems to proceed from a contagion of a specific nature and singular quality, adds, that it does not, like most other contagions, necessarily produce a fever; nor does it, like most others, occasion any eruptions, or produce otherwise any evident change in the state of the human fluids. From our own observation and feelings, we are led to think, that there is always more or less fever, and not unfrequently eruption also. In small pox and measles we can discover eruptions in the roof of the mouth and fauces, and those eruptions probably extend farther than we can see. They may, like apthæ, sometimes extend throughout the

whole of the alimentary canal." This view of the pathology of whooping-cough, although presenting an appearance of ingenuity, is, we believe, on the whole, incorrect, since dissections of those who have died of the disease, have revealed no such appearances, (at least in a very large majority of instances) as apthæ, or indeed any other species of eruption on the mucous tissue of the stomach and intestines. That the mucous glandulæ, abounding so plentifully in those membranes that are the seat of the disease, may be, and sometimes are, considerably enlarged, is a fact that cannot be denied; but this circumstance is widely different from the existence of any specific eruption, and depending merely on the encreased excitement, or to speak the language of the day, on the irritation of these parts.

We pass by our author's observations on the "Breathing and Vocal organ." In this part of his essay he gives an anatomical description of these organs,—an account of the mechanism of the voice,—a physiological explanation of the different species of coughs, and a disquisition on dentition,—the whole intended to show, "that irritation and morbid affections of parts below the thorax, even as low down as the bladder and hemorrhoidal veins, produce a convulsive action in the breathing organs, expressed by a cough."

The next chapter is devoted to the subject of "Dissections, and the appearances in diseased bodies after life." Its insertion here is, we believe, improper, as occasioning much repetition that might have been avoided.

He mentions, that in some dissections recorded by Dr. Watt, there were found long patches of inflammation on the small intestines, and on the external parts of the stomach, looking as if painted with coffee grounds; and a universal crust of lymphatic exudation on the internal surface, with here and there streaks of red;—these appearances, he admits, evince a pre-existing state of inflammation. Now it appears from the dissections of the same Dr. Watt, that in a great majority of instances, the lungs were discovered to be the seat of violent

inflammation, or its consequences, effusion, adhesion, suppuration, &c. Consequently, if Dr. Waterhouse feels disposed to deny the evidence presented by these dissections, of the existence of disorganization, or of the lungs, we cannot understand how he can venture to maintain, that some dissectors have shown redness, &c. in the intestines, and that these prove a pre-existing inflammation in these parts, the evidence should be deceptive or satisfactory in both cases.

Dr. Waterhouse next enters on the *nature of whooping-cough, deduced from the operation of certain remedies,* and endeavours to adduce, satisfactorily, evidence of the location of the disease in the alimentary canal, and not, as is believed by many, in the pulmonic organ. "Have not all those medicines most efficacious in mitigating the severity of whooping-cough, a special reference to the alimentary canal, rather than to the trachea, bronchia, or pneumatical system of the lungs?" In this belief of the possibility of discovering the seat of whooping-cough from the operation of remedies found beneficial in it, Dr. Waterhouse has merely repeated the opinion of Pinel, who, after referring the seat of this disease to the stomach, says, "From this arises the utility of evacuants, antispasmodics, and tonics, in subduing the extreme sensibility of the digestive organs. We deem it almost superfluous to point out the fallacy of this belief, and the erroneousness of the assertion, that a disease is of gastric origin, because evacuants are found of advantage in its cure. We need only remark, that on the same principle, croup should be regarded as a violent disease of the stomach, because emetics and cathartics are used in it with the most decided benefit.

Among the remedies employed in the treatment of the whooping-cough, our author first notices emetics, which he thinks always give temporary relief. In this opinion we entirely concur. Notwithstanding their proscription by those who asserted they were fatal, emetics must be regarded as remedies of great efficacy in pertussis; and in this light they have been considered by all intelligent writers on the subject. Not to

mention names of an older date, we observe that Burton employed them whenever the primæ viæ were loaded with phlegm, by which he thought the absorption of remedies in the lacteals was prevented; and by Buttu they were judiciously regarded as the natural cure of the disease. But emetics do not act beneficially in whooping-cough simply as evacuants, but also by making a strong impression on the system, by determining to the surface, and producing a counter irritation in the stomach, by which they contribute to mitigate the violence of the inflammation, or even shorten its duration. In order therefore to derive from them their full effect, they should be repeated often, avoiding, however, their exhibition whenever the stomach is found to be in a state of irritation, as is frequently the case in this malady. But although emetics generally are of decided advantage, some choice exists in respect to the particular article to be employed. By many, the emetic tartar is preferred, whilst by others, ipecacuanha has been regarded as possessing a more decided efficacy. Both of these medicines are good, and may be used to meet different indications. Thus, whenever it becomes necessary to evacuate the stomach of its foul contents, and this organ gives signs of being too highly irritated to admit of the exhibition of powerful remedies, the ipecacuanha may be used with the greatest advantage, "because," as Dr. Pierson has judiciously remarked, (Prize Essay, p. 13,) "no quantity which can be easily swallowed at once, has ever been known to produce a deleterious effect,—a charge to which the preparations of antimony are all liable when given in full doses." On the other hand, antimonials will be found more serviceable, whenever the necessity of a strong and revulsive action will be fairly indicated. Tartar emetic, in small doses, combined with some testaceous powders, and an occasional dose of magnesia at night, were the principle remedies recommended by Fothergil; and he derived from them the most decided benefit in the cure of the whooping-cough. We recommend the following remarks in respect to the time of administration, as well

worthy of attention. "This time of day (say between breakfast and dinner,) has been fixed upon preferably to any other, on these considerations, viz:

If the puke is given early, the stomach being empty, a straining too great to be borne without detriment might be occasioned. Was it given in the evening, the child would, perhaps, be robbed of too great a quantity of nutriment."

Some physicians have recommended nauseating doses, and cases occur in which this practice may, perhaps, be sometimes beneficial. Frederick Clossius recommended highly the golden sulphuret of antimony, in quantities so small as not to be productive of any action on the stomach; and in France, according to Bosquillon, the ipecacuanha and Kermes' mineral were prescribed in a similar manner. But he very wisely remarks, that these medicines do good only when they excite vomiting; and in general we think it will be found that nauseating medicines distress children too greatly to admit of their general exhibition.

Next to emetics we shall speak of fumigations. Of the many substances employed for that purpose, tar is the most common, and found peculiarly beneficial in relieving, by the vomiting it excites. Dr. Watt speaks in terms of praise of this remedy, and other writers have corroborated his statement of its benefits. The tar may be employed, either simply, by filling the room with its fumes, procured by placing a dish filled with it in hot coals, or else in the manner recommended by Dr. Wansborough of London. The method of this gentleman, which is far preferable to any other, consists in placing some tar in a vessel having a tube for the passage of the fumes. A heated poker is next introduced into the tar, and the fumes issuing through the tube are made to be respired by the patient. In respect to the *modus operandi* of tar fumigation, we cannot agree with Dr. Waterhouse, who, forgetting that the remedy has been employed with success by Crichton and others, in genuine affections of the lungs, remarks, that the tar fumigation proves that the seat of the disorder may be any

where rather than in the mucous membrane of the trachea and bronchiæ. The reverse is, we believe, the truth; nor can we acquiesce in the assertion that the larynx is at all times a sentinel that instantly repels such an invasion. The fact that these fumigations are beneficial in relieving oppression arising evidently from wrong action in the lungs, proves that this sentinel is not so vigilant as our author would seem to think, and does not so invariably repel the invasion. Not content with this, our author adds, although in so doing he evidently contradicts himself, that this repulsion takes place more especially in this disease, when the larynx is *rendered more sensitive by a peculiar inflammation.* In all cases of *whooping-cough there is an increased sensibility in that portion of the mucous membranes covering the larynx, and pharynx,* which repels acrid fumigations, and *creates trouble in swallowing dry substances.*"

The next medicines Dr. W. mentions as affording relief in this disease, are purgatives. And because the venerable Heberden tells us, that "in disorders of the lungs, emetics frequently procure easy breathing, but that cathartics are so very seldom useful, as scarcely to deserve to be even tried;"—he naturally concludes, "that experience, in opposition to theory, places the primary seat of the distemper in the alimentary canal." Dr. Waterhouse seems to be an advocate for mild purgatives in the treatment of whooping-cough. "Late writers," he says, "recommended strong, and often repeated cathartics; but neither sound practice, nor legitimate theory, can justify the procedure. Parents, and also young practitioners, have recourse, rather too often, to drastic purgatives in the ordinary complaints of children. This practice destroys the healthy balance between the nutritive and absorbent systems; and if it does not lay the foundation of scrophula, it certainly developes a scrophulous habit in some who would otherwise have passed through life without such appearances." These remarks on the dangers attending a too violent system

of purgatives in the diseases of children in general, and in whooping-cough particularly, must be regarded as judicious; for although a discharge from the alimentary canal proves beneficial in a great number of their diseases, still the frequent and sometimes daily administration of violent purgatives cannot but prove dangerous in many instances, by producing an undue and too long continued irritation in the mucous tissue on which they are applied, and in this way laying a foundation for numerous and fatal maladies. Of the purgatives employed in this disease, we have generally derived most benefit from the calcined magnesia, or castor oil, either administered alone, or preceded by small doses of the submuriate of mercury. This latter substance should never be overlooked when the alimentary canal is disordered.

By Sydenham, bleeding was earnestly recommended in the whooping-cough. "By this practice," he said, "of venesection, and repeated purgatives, and by this alone, is conquered the convulsive or whooping-cough, and obstinate disorders which scarcely any other method will subdue." We believe that few physicians will be found to refuse their testimony in favour of the occasional advantage derived from blood-letting, in the treatment of this disease.

Cases are frequently met with in which symptoms of excitement run so high as to necessitate the prompt and free use of the lancet; but in a majority of instances emetics, cathartics, and the remedies we shall mention in the progress of our review, will be sufficient to arrest or mitigate the disease, and to do away the necessity of blood-letting. "We have seldom resorted to the lancet in whooping-cough," says Dr. Waterhouse, "unless some accessary symptom, in plethoric subjects, has urged it; as to obviate a threatened apoplexy, or where there has been some great and sudden exposure to cold and wet, inducing pleuritic inflammation. This is, however, an accidental disorder, superinduced on the original regular distemper."

Leeches or cups, we have known to be very serviceable in this disease. Faithful to the views he has adopted, in respect to the seat of the whooping-cough, Mr. Webster (Lon. Med. and Phys. Journal) recommends leeches to be applied to the forehead, and behind the ears, "for there the blood being drawn from parts supplied by the ramifications of the supraorbital arteries, and of those of the internal ear, it is drawn more directly from the head than it would be were the leeches applied to the temple." Upon the same principles, he thinks, the jugular vein may be opened with advantage; though he has not had recourse to this operation in any case.

We have no doubt, that applied to these parts, leeches will occasionally prove of great service in the cure of the whooping-cough; but we would limit their employment in this way, to those cases in which symptoms of irritation or congestion of the head are manifested—in those instances in which blood, during the paroxysm, rushes to the head in too great quantity, as well as in those attended with convulsions. But such symptoms are not invariably found; and in the generality of instances, leeches and cups to the *chest* will be more serviceable in subduing or mitigating the inflammation of the mucous membrane of the bronchiæ and trachea, or parenchyma of the lungs.

In this stage of the disease, (first) blisters and other external stimuli have been had recourse to, and from them the most decided benefit has been derived. In the generality of instances, indeed, blisters will do away the necessity of bleeding, and may be applied early in the disease. "Under the impression," says Dr. W, "of whooping-cough being a simple inflammatory affection, *blisters* have been applied after bleeding and purging. They were used by Willis, and approved by Cullen. Those who conceived it to be a spasmodic disorder, had recourse to blisters and to embrocations, in which camphor, turpentine, oil of amber, and garlic, have predominated. These, by exciting a counter irritation, have,

now and then, afforded temporary relief, and, with change of air, effected cures. Few things have been more extolled in Europe, than a solution of tartar emetic, mixed with a strong tincture of cantharides, and used by embrocation; which it seems gives relief in proportion to the violence or rage of the eruption produced by it on the skin."

In the second stage of the disease, when by the remedies we have already mentioned, all symptoms of high inflammation have been removed, cantharides have been administered internally, and received high eulogiums for their beneficial effects. So early as the year 1735, the practice was introduced by Burton, who used them in combination with bark and camphor, in a mixture containing a small quantity of the balsam copaiba. By Sutcliff, the remedy was employed in the liquid form, and in this way it is better suited for children.—Lettsom used it also with success, as well as Millar and Hillary. Whether administered in the solid or liquid form, a certain portion is given three or four times a-day, until a slight stranguary is excited; and this is considered the criterion of its efficacy.

Dr. Waterhouse, although entertaining the opinion that the internal, as well as external, use of cantharides, is sometimes productive of great inconvenience and distress in causing stranguary, especially in hot and *very dry* weather, says, nevertheless, that there can be little doubt as to the efficacy of cantharides, and balsam capivi, in relieving, and sometimes curing whooping-cough, "and its œtiology," he continues, is not very difficult. "They both stimulate, if not vesicate, principally, the membranes of the urinary organs, from the kidneys to the urethra. The effect is most felt in the neck of the bladder, and also, probably, in that irritable point in the internal surface of the bladder, mentioned by that eminent anatomist, Charles Bell, directly over the junction of the muscles of the ureters, with the internal sphincters of the bladder; the point which, when inflamed, provokes an in-

cessant call to make water. If, after the constant use of these powerful medicines, we could see the mucous membrane of those patients, I have little doubt, but we should find them efflorescent throughout, and in proportion to this efflorescence, would probably be the diminution of that pathognomonic one in the œsophagus, diaphragm, larynx, and upper orifice of the stomach."

Remedies included in the class of antispasmodics, have all at various times been employed in the cure of the whooping-cough. Whether or not their efficacy may be accounted for on the principle of a new, strong, and ungrateful impression on the membranes, rendered highly sensitive by disease, as asserted by our author, we shall not pretend to decide. True it is, however, that these remedies, and more particularly the assafœtida, are of great benefit in removing or relieving the spasmodic cough that remains after the subsidence of the more inflammatory symptoms, and constitute, after the employment of the antiphlogistic remedies, the principal means employed by the most enlightened and experienced physicians of this country.

Castor was introduced in practice with much commendation, by Dr. Morris (Lon. Med. Obs. and Inquiries, Vol. III. 281.) In combination with bark it produced, according to his report, considerable alleviation of the whooping and retching attendant on this disease. But subsequent trials, by other practitioners, do not seem to have confirmed the accuracy of his report, regarding its beneficial effects. The musk has also been recommended, and was used by Hillary for the purpose of relieving the convulsions which sometimes supervene in this disease. The artificial musk was also introduced; but these medicines, like the castor, have lost the little and ephemeral reputation they had at one time acquired.

In this state of whooping-cough, tonics, both of the vegetable and mineral kind, have been employed by different practitioners, and in many instances with the most decided

advantage. Among the former, the Peruvian bark stands conspicuous. By Burton it was said to be as successful with him in this disease, as in the cure of the ague. On the continent of Europe it was used with success, and received the praises of Rosenstein, Whytt, Bisset, and many others. Speaking of its virtues, Dr. Cullen says, "I consider the use of this medicine as the most certain means of curing the disease in its second stage; and when there has been little fever present, and a sufficient quantity of the bark has been given, it has seldom failed of soon putting an end to the disease."

The nitrate of silver has likewise been employed, with what success we hardly know;—the remedy having been made little use of in this country. Its violence would seem a sufficient reason for discarding it from practice among children. Ferriar and Simmonds used the arsenical solution with advantage even in cases of young children.

By Dr. Forbes, of Edinburgh, the internal use of lead has been recommended. He employed the liquor sub-acetatis, or Goulard's extract. Notwithstanding, however, the praises he bestows on this medicine, we would caution our young readers against its too *bold* administration, owing to its giving rise, in many cases, to painful and sometimes dangerous affections of the bowels. The same remedy seems to have been used in France for a long time past, in combination with lischen pixidatus. (See Le Medecin de la Campagne, Vol. II. p. 290.)

The various narcotics have been extensively tried in the second stage of this disease. Although having the testimony of Hufeland and of Demachy in its favour, opium has, in general, been found of not much utility in this disease. Dr. Willan, however, whose authority is of considerable weight in medicine, found the watery solution of this substance very serviceable in mitigating the paroxysms, and prolonging their intervals. We have often had recourse to the various preparations of opium, and have never derived from their use much decided

advantage. Digitalis has been regarded as much superior to opium, and consequently prescribed pretty extensively in this disease. Of this article Dr. Pierson* speaks favourably. "Six drops," he says, "of a saturated tincture, may safely be given to a child one year old, at intervals of six hours. This dose may be increased by one drop at a time, till a diuretic effect is produced, or the state of the pulse indicates its diminution or suspension." Hemlock has also been used in this disease; but notwithstanding the high encomiums bestowed upon it by Dr. Butter, it has been found of not much value. By many, Belladona has been used. Shoeffer, Hufeland, and Wetzler, in Germany, and Alibert and Janin, in France, speak favourably of its virtues.

Dr. Granville, of England, as well as some physicians of this country, bestow praise on the use of the prussic acid; whilst, on the contrary, Dr. Elliotson, of Europe, and Dr. Pierson, of Salem, have not found it to display much power over this distressing complaint. We have given it a trial in several instances, and have never derived from it any decided benefit.

Some popular remedies have been sometimes found of advantage in the whooping-cough. Among the negroes of the West Indies, castor oil and molasses mixed together, and administered often in the day, are said to afford effectual relief from the distressing symptoms of this complaint. "Equal portions of linseed oil and flower of sulphur," says Dr. Waterhouse, "have been used with remarkable success in mitigating the violence of whooping-cough. Some people consider it the sovereign remedy in this distressing complaint. A tea-spoonful of this mixture, taken three or four times a-day, or less frequently, according to the age of the subject, has certainly produced beneficial effects."

Change of air has always been regarded as peculiarly beneficial to patients suffering under the whooping-cough. In-

* Prize Essay.

stances are not wanting to prove that children transported many miles from home, have insensibly lost their convulsive cough, and from a state of complete languor, and too sick to take nourishment, have revived after a day's journey, and sucked with relish.

"But breathing another air," Dr. Waterhouse remarks, "comprehends not all the advantages resulting from a journey. Transport a man fifty miles, in a close carriage, while asleep, and unconscious of his removal, merely to breathe an air hanging over another soil, he probably would gain but little advantage from it. It would hardly alter the morbid sameness of thought and feeling. But let his migration be through a varied country, diversified with hills, vallies, rivulets, plains, and forests; so journeying in places different from the one he long inhabited; resting in houses unlike his own; meeting people differing in manners and customs from his; partaking food differently cooked; and drinking water from a different soil; these things, added to the exercise, insensibly operate a change in mind and body, and generate healthy feelings in the adult valetudinarian, especially if he be hypochondriacal; and they have more or less effect on children; for pleasing novelty is a healthy stimulus."

"It appears, that those who are suffering in the country, under asthma, or whooping-cough, gain the same benefit from removal into the city, as the inhabitants of a large town, similarly affected, reap from removing out of it. We have observed that children go from one country village to another, twenty or thirty miles distance from the salt water, with the same favourable result. They go from the Highlands to the low fishing towns, and from the margin of the broad Atlantic to the mountains, with equal good effect. In these cases, it is in vain to talk of the good air, or the bad one, of certain places. It is the change; it is the new impression that overcomes the morbid one."

Dr. Waterhouse next treats of the "*Analogy between Whooping-cough and Dysentery.*—"I have long enter-

tained the opinion," he says, "that if whooping-cough and dysentery were not very near akin, they were in that degree of relationship, as to bear traits of a family likeness. Both infest the mucous membrane; both affect, especially, the alimentary canal as one *great gland*, beginning at the pharynx, and ending at the rectum. Both are accompanied with violent convulsive affections of the diaphragm; both are derived from contagion; and both, we suspect, are exanthematous affections of the inverted skin, or mucous membrane of the œsophagus, stomach, and intestines."

After this declaration, the author notices the cases recorded by Dr. Watt, and observes, that whenever in these the whooping-cough had been violent, or near the fatal close, the discharges from the bowels were very dark and fœtid, with an appearance of small fragments of membranes diffused through them—that in most cases, where the bowels had been very lax, involuntary discharges, great irritation, and a prolapsus ani have accompanied the paroxysm. Again he remarks that "they could never pronounce the patient out of danger, until they had procured something like the natural fæces. "Here," he exclaims, "the resemblance between the dysentery, and disease under discussion, is striking. Would it be straining the analogy to say, that what tussis convulsiva is in the one, tenesmus is in the other. By which we mean only, so nearly resembling, as to refer them both to the same law of the system, whenever the mucous membrane of the alimentary tract is principally affected."

We are indeed at a loss to ascertain what Dr. Waterhouse pretends to have discovered. It seems to us, that all he has proved in the foregoing observations, amounts to this fact, and no more, that whooping-cough is the effect of the irritation or inflammation of one portion of the mucous membrane, and dysentery of the inflammation of another portion;—that in cases of the former disease, dysenteric symptoms occasionally occur; or in other words, that the irritation, constituting

the whooping-cough, extends to, or attacks at the same time, the living membrane of the intestinal tube, and complicating itself with a spasmodic action of the muscular fibres of that canal, gives rise to symptoms of the dysentery. Than this, nothing can be easier to conceive; and to prove it did not necessitate so much labour, since all physicians, familiar with the laws of irritation, are perfectly aware of the phenomena of its extension. In our opinion, therefore, the same analogy exists between whooping-cough and dysentery, as between catarrh and the same disease; both are the effects of inflammation seated in the mucous membrane, but modified by the nature of the part affected.

In respect to his favourite hypothesis of internal exanthematæ, and his opinion that dysentery is a distemper belonging to that class, we shall say little, since he has not adduced one single remark or fact calculated to support the assertion. His quotations from Aretœus, Bonetus, Dolœus, Cleghorn, Fraser, Donald Monro, Sir George Baker, and Linnæus, who he poetically denominates the "Celebrated Secretary of Nature," amount to the fact, that in dysentery the intestines are highly inflamed, and are found, on dissection, to contain a greater or less number of ulcers, or ulcerated tubercles; and not at all that there existed a specific exanthematic disease. Were ulcers signs of a disease of the latter class, then chronic catarrh, consumption, typhus, and other kinds of fever, diarrhœa, and, indeed, almost all diseases attacking the mucous tissues, and which, in the true spirit of a zealous nosologist, he undoubtedly regards as totally distinct in essences from dysentery, would depend on the same cause, or probably on a different species of eruptive disease. We hope Dr. Waterhouse will not venture to maintain such an opinion, and we are really sorry he has asserted as much as he has done on the subject of these ulcers; since all his arguments and reasoning have only tended to prove satisfactorily to us, that he has neglected to avail himself of the labours of our modern

transatlantic brethren, and of some of our own countrymen, and thus failed to keep pace with the improvement of knowledge.

The next and last portion of this work to which we shall direct the attention of our readers, is his chapter *on the power of vaccination in removing or mitigating Whooping-cough.* On this subject we are of opinion, that the observations of Dr. Waterhouse are entitled to considerable attention. His indefatigable researches on vaccination, are sufficiently known; and we are not a little pleased to learn the results of his extensive experience on the power of small-pox and kine-pox in removing or mitigating pertussis.

On this subject a great diversity of sentiment is discovered in the writings of those who have made the whooping-cough and vaccination the objects of their investigations. By some, the fact of small-pox, and the vaccine disease, coexisting in some instances with the whooping-cough, has been regarded as a strong argument against the opinion of a preventive power in vaccination against pertussis; and Dr. Pierson, in his essay on this latter disease, states, that he has never, in his numerous experiments, witnessed the fortunate results attributed to vaccination. Mr. Bertrand, on the contrary, arrives at a very different conclusion from his experiments; and Valentine, in his work on vaccination, and in his biographical notice of Jenner, lends support to the opinion of Bertrand. He remarks, that he witnessed many cases in Nancy; and says of Jenner, that he had noticed several times, that if we vaccinate after the first twelve or fifteen days of the whooping-cough, the disease is mitigated, and is finally cured more promptly. Husson, in his "*Recherches sur la Vaccine,*" quotes from the report of the Commission of Milan, the case of a girl who, during eighteen months, was afflicted with a convulsive cough, which disappeared as soon as the inflammation of the vaccine had gone through its different stages.

He likewise mentions, that many children, affected with whooping-cough, were vaccinated at Brussels, and experienced relief proportioned to the development of the vaccine disease. "I was once sanguine on this head," says Dr. Waterhouse, "and believed almost all I wished; so that, when my children were suffering under whooping-cough, I regretted they could not be revaccinated. Subsequent experiments made me less sanguine, but have not influenced me to relinquish entirely the opinion, that the variolous, as well as vaccine fever, diminishes greatly, and sometimes entirely obliterates the symptoms of the epidemic convulsive cough.

"I have never had repeated positive proof that vaccination arrested and subdued the distemper in its first stage; that is, before the eighteenth or twentieth day. In some few cases I thought it prevented the absolute formation of the disease, when there was reason for believing that the subject had been infected by the contagious cough. But the sagacious physician will here see the chances for deception.

"I feel more confident, that after the distemper had reached its acmé, and was on the decline, that then vaccination, when pretty strongly marked with febrile symptoms, has operated an alteration for the better. That small-pox would modify measles, is not a new idea; and that it has modified whooping-cough I have no doubt. Since vaccination has chased away small-pox, some think that measles and whooping-cough have increased in frequency, and malignity.

"I am of opinion, from observations, that children vaccinated in the spring, or early in summer, are not so liable to cholera in the autumn of the same year. In this, I believe, I am not deceived; yet its confirmation, or rejection, is left to the experience of others." R. L. R.

ART. III.—*Characteristic der Franzoesischen Medicin, mit Vergleichenden hinblicken auf die Englische.* Von JOH. LUDW. CASPER, M. D. &c. &c. Leipzig, F. A. Brockhaus. 1822, Octavo, pp. 608.

Characteristic of French Medicine, &c. &c.—It requires no very extensive acquaintance with medical literature to perceive, that medicine, both in relation to its theory and practice, exhibits national features as characteristic as the physiognomy and manners of nations themselves. In no country, perhaps, do we find this fact more strikingly illustrated than in France. The Parisian school, especially, exhibits the most obvious peculiarity in this respect. It possesses, as our author very correctly observes, an experimental, matter-of-fact character, in which we perceive very little of the spirit of philosophical generalization, or of refined speculation upon the nature of phenomena. The French have carried into medicine the philosophy of *Condillac.* The senses mark out to them the limits of inquiry. The dissecting knife and the crucible, conduct them to the end of their researches. They rarely carry the "mind's eye," beyond the phenomena which these disclose to their senses; and the fields of recondite inquiry,—of deep abstract speculation are but seldom entered. It is from this cause that, with the exception of the writings of Broussais perhaps, we look in vain for any such thing as a system of *general pathology* in French medicine. The French are indefatigable observers of phenomena; they are continually enlarging the sphere of our knowledge by experimental researches and minute examinations of the *sensible* conditions of the animal system; they are industrious in augmenting the mass of materials, whilst but very few attempt to work up these materials under the plastic powers of the mind into a regular and connected edifice of science. With the Germans the thing is quite different. With them the philosophy of *Kant* is the

mens quæ agitat molem. Whilst they are the most industrious people under the sun, in collecting and recording facts and observations, they are also the most addicted to that refined and sublimated speculation which characterises the Kantean school. They consider it not enough to be acquainted with phenomena and their obvious relations;—they look for higher views than those which the senses afford;—they endeavour to find out the hidden relations of phenomena,—to penetrate into their nature, and to draw from them general and fixed principles. Hence their medical literature abounds so much with works on *general pathology*.

In relation to these two distinct traits of the German and French schools of medicine, that of the English holds a middle rank. Its character is neither so speculative as German medicine, nor so experimental as that of the French. The English devote less attention to the mere observation of phenomena than the French, and waste less time in speculating on them than the Germans. Their logic is more searching than that of the former, and less vague and wandering than that of the latter. The *inductive* mode of philosophizing is more closely pursued by them than by either of the other two nations. Without, however, pursuing this topic any further, we pass on to a review of the work before us, which, in a style at once pure, elegant, and perspicuous, presents a very full and striking view of the characteristic traits of French medicine.

In the second chapter, Dr. Casper gives a circumstantial account of *the internal police of the French medical schools, and of the course and plan of instruction prescribed by government.*—The old system of the French universities, which had existed, with very little variation, for more than seven hundred years, was in 1820 dissolved, and a new one raised in its place, which, in its whole organization, shows the influence of a jealous power, whose entire policy is dictated by a spirit anxious for the perpetuation of those

principles which are to fix the stability of thrones, and arrest the progress of just and liberal sentiments. That our readers may have an idea of the severe discipline and *surviellance* to which the students of the French universities are subjected under the present regulations, we give the substance of the principal ordinances in relation to this subject.

The *Commission* of public instruction have ordained:

1. That in order to obtain the degree of doctorate, the student must obtain a *certificate of inscription* from the faculty which he attends. This certificate must be exhibited by the student when he presents himself at the lectures of the faculty.

2. Admission may be gained to the public lectures without being regularly inscribed, by means of an *admission card*, obtained from the faculty. For this purpose a register, signed by the dean and secretary, is kept, into which those who wish to obtain an admission card must enter their names, age, place of birth, and actual residence. If they do not reside in Paris, they must exhibit a certificate of permission to remain in this city. Admission cards to the lectures can be denied to no one who has complied with these regulations.

3. Every one who attends the lectures of a faculty, must, when requested by the dean or the professors, exhibit his *inscription* certificate, or his card of admission. This is noted on the card or certificate, which is then again returned.

4. If any disturbance is created by a person holding an admission card, the card may be annulled at the request of a professor and the dean, by a resolution of the faculty. If any one is detected in lending his admission card to another, the person so detected, loses his card. If an admission card is annulled, the possessor shall be requested by a written note, to return it to the secretary within three days. If he neglect doing so, his name, with the number of his card, shall be put up in the halls of the faculty, and the janitor shall deny him entrance if he presents himself for admission. The person

who has thus lost his card, may apply for a new one; the faculty determine whether he shall have another card or not.

No expense is incurred either for a regular inscription, or for a card of admission. The cards must be renewed at the beginning of every new course of lectures.

By the king's decree, issued in July, 1820, it is ordained that:

1. After the 1st of January, 1821, no one shall be admitted to the first inscription in any medical faculty, who has not previously obtained the degree of *Bachelor of belles-lettres.* (Bachelier—és lettres.)

2. From the 1st of January, 1823, no one shall be admitted to inscription in any medical faculty, who has not previously obtained the degree of *Bachelor of Arts.* (B.—és Sciences.)

3. From the 1st of November, 1823, every one who applies for a *first* inscription in any medical faculty, or secondary school, must exhibit:—1. A certificate of birth;—2. If he is a minor, a certificate attesting the consent of his parents or guardian to his attending medical lectures; this certificate must designate the place of residence of the parents or guardian. —3. Finally, the diploma of the requisite bachelor's degree.

4. From the 1st of November, 1823, no one shall be admitted to inscription in a faculty or school, which is located in any other town than that in which the parents or the guardian resides; unless he be presented by a person who lives within the district of the faculty or school; and which person is then obliged to enter his name and place of residence in a register kept for that purpose. The student has then his domicil with this person, and to which all notifications relating to him must be sent. Should this person, who stands in the place of the parent or guardian die, or go on a journey, the student is bound to obtain another in his stead, otherwise every inscription which he may have obtained since the death or absence of his sponsor shall be annulled.

5. The student, also, must give in his actual place of residence;—and whenever he changes this, he must notify the faculty of the change. Every false notice, or neglect of notice, may be punished by annulling his inscription.

6. Every professor in a medical faculty, or secondary school, is bound *to call out the names of those students who attend his lectures at least twice every month.* If the number of students is too great to be called out at one time, the professor may call out the names of a part of them daily, so that all may be called up at least twice in each month. No notice is to be given of the day on which the names are to be called out. If any one is detected in answering for another, he shall lose his inscription. Every student who has been twice absent in a quarter of a year, without a proper excuse, shall have no *attest of his industry* from the professor.

7. Inscription certificates are only given for the three months in which the student has obtained *attests of industry* for *all* the lectures which he is attending.

8. Every want of respect, every act of insubordination in a student, towards a professor, shall be punished with the loss of one or two inscriptions. In case of a repetition of the delinquency, the student may be excluded the lectures for a term not less than six months, nor more than two years. The same punishment shall be inflicted on those who excite the other students to disturbances within the schools.

Every student who shall excite, or be the cause of disturbances *without* the walls of the schools, or who may take part in any public disturbances, shall be punished with the loss of not less than two, nor more than four, inscriptions, or be excluded from the lectures, for a period of not less than six months, nor more than two years. The student may appeal to the commission of public instruction. A repetition of the offence, is punishable by an exclusion from *all* the schools for a term extending from six months to two years. This exclu-

sion from all the academies is dictated by the commission of public instruction. The student may appeal to the council of state.

The students shall form no associations of any kind whatever, without having obtained the consent of the local authorities, and without having previously informed the rector of their intentions and object. They are also prohibited from acting and writing as a collective body.

The following course of studies is prescribed for those who desire to obtain the degree of Doctor of Medicine.

First year. First, Second, Third, and Fourth Inscription.	Winter.	Anatomy Physiology Chemistry.
	Summer.	Hygeiene External Pathology Botany.
Second year. Fifth, Sixth, Seventh, and Eighth Inscription.	Winter.	Anatomy Physiology Operative Surgery.
	Summer.	Hygeiene Pharmacy External Pathology Chirurgical Clinic.
Third year. Ninth, Tenth, Eleventh, and Twelfth Inscription.	Winter.	Operative Surgery Chirurgical Clinic.
	Summer.	Internal Pathology Materia Medica Medical Clinic.
Fourth year. Thirteenth, Fourteenth, Fifteenth, and Sixteenth Inscription.	Winter.	Medical Clinic History of Medicine.
	Summer.	Internal Pathology Forensic Medicine Clinique de Perfectionnement Midwifery.

The following is a list of the Professors and Officers belonging to the established Medical Schools of Paris.

Leroux, Perpetual Dean, and Professor of Clinical Medicine.
Boyer, Chirurgical Clinic.
Chaussier, Anatomy and Physiology.
Deyeux, Medical and Pharmaceutical Chemistry.
Dubois, Clinique de Perfectionnement.
Hallé, Hygeiene.
Lallement, Operative Surgery.
Pelletan, Examinator, Internal Pathology.
Richard, Examinator, Botany and Natural History.
Thillaye, Keeper of the Museum.
Desgenettes, Adjunct to Halle.
Dumeril, Secretary of the Faculty, Anatomy and Physiology.

De Jussieu, Botany.
Richerand, External Pathology.
Vauquelin, Chemistry.
Desormeaux, Midwifery.
Dupuytren, Chirurgical Clinic.
Moreau, Librarian, Medical Literature.
Royer-Collard, Examinator.
Béclard, Adjunct to Dubois.
Marjolin, Surgery.
Orfila, Toxicology.
Fouquier, Internal Clinic.
Roux, Surgical Clinic.

Besides these professors whose lectures the student is obliged to attend, there are other lecturers of great eminence in Paris, attached to other institutions. Thus in the *Jardin des Plants*, Cuvier, Lacepede, Blainville, Decandolle, and others. In the *College de France*, Thénard, Portal, Biot, and others, and in the *College du Plessis*, Gay-Lussac, Hauy, &c.

Among the private lecturers Esquirol, Alibert, Magendie and Broussais, are the most distinguished.

No student is permitted to offer himself for examination, without being able to exhibit the sixteen certificates of inscription, given out during the course of instruction. If, during his attendance, he has forfeited any of these certificates, he must continue his attendance until he has obtained the full complement, before he can be admitted to an examination. The candidate for a degree has to undergo *six examinations.* These are conducted publicly, and each lasts one hour. After having passed successfully through his examinations, the candidate hands in his *dissertation,* which he defends publicly, as is customary in the colleges of other countries.

It is manifest, says our author, from a view of the course of studies designated in the above table, that, in France, the interests of surgery predominate greatly over those of medicine.—Five seasons are devoted to anatomy; two to *external* or surgical pathology; two to operative surgery; three to surgical clinics, and one to midwifery; whilst every thing which relates to the study of internal diseases is

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limited to two seasons only; nay the *Officiers de Santé*, are required to attend but one season to these studies. No lectures are delivered on general pathology; and what is more remarkable, the lectures on *internal* nosology are limited to two courses.

We pass on to the fourth chapter, which is devoted to an account of the infirmaries, dispensaries and hospitals, located in the French metropolis. The number of these institutions in Paris is very great. More than 35,000 patients are annually received into them.

The *Secours à domicile*, of which there are twelve in Paris, are public charitable institutions for the relief of the poor and sick at their houses. These dispense food, clothing, wood, &c. to the poor, and supply them with medical attendance and medicines. Besides these public *charities*, there are a great number of private associations, by which medical and other relief is afforded to the poor. The *Societé Philanthropique* alone supply medical attendance, &c. to about 2000 patients annually.

Maisons de Santé; these are not properly speaking charitable institutions, although their usefulness in so populous a city must be very extensive. They are designed as private hospitals, in which, persons who have no regular homes, or who want the conveniences of proper attendance, may obtain them at a moderate expense. The author counted fifteen of these institutions. They are all located in the remotest suburbs, and are generally environed by pleasant gardens, and removed from all tumult and noise.

The *Hospices*, resemble our alms-house infirmaries. There are 12 of these in Paris. In 1819, the number of invalids received into these *Hospices* amounted to 18,260.

Paris contains ten *Hospitals*, properly so called, all of which derive their revenue from government. They are, in fact, integral parts of the government, and are placed under a *central administration*, which exercises a general superin-

tendance, not only over the hospitals and *hospices*, but also over the above named benevolent institutions.

The *Bureau central d'admission*, which is under the immediate direction of four physicians, is as it were the initial point of the hospitals. Here *all* must apply, who wish to gain admission, as patients, into a public hospital. The central administration determine according to the kind and degree of the disease, to which of the hospitals the applicant shall go. The central administration is always accurately informed of the number of disposable beds in these institutions; for the local superintendants of each hospital are obliged every morning, after the physicians have been round the wards, to render an exact account to the *central administration*, of the number of patients, of unoccupied beds, of deaths, admissions, discharges, &c. &c. The central Bureau is itself an institution, in which medical advice and medicines are given gratis to the poor.

In the Parisian hospitals the sick are separated into classes, according to the general character of their diseases, and placed in distinct wards. They have also their (*Salles de Medicine*) medical wards, and (*Salles de Chirurgie*) surgical wards. Several of the hospitals are exclusively devoted to the reception and treatment of certain diseases, and which, with but very few exceptions, are not admitted into the other public institutions of this kind. Thus, venereal patients are admitted only to the *Hôpital de Capucins*. Those affected with chronic cutaneous affections, are sent to the *Hôpital St. Louis*. Children to the *Hôpital des enfans malades*. Epileptic and maniacal patients have their appropriate institutions; and those affected with colica pictonum, and consumption, are for the most part sent to *La Charité*.

The attending physicians and surgeons make their visits daily at six o'clock in the morning during summer, and at seven o'clock in winter. They enter the wards, dressed in long white aprons, and begin by calling out the names of those

(eléves) pupils who are attached to the institution, and whose duty it is to attend them in their visits round the wards. Two of the *eléves* are appointed to the task of recording the dietetic and medical prescriptions in separate books, as they are dictated by the prescribing physician at the bed side. The physician then signs his name at the bottom of the day's prescriptions, without which no attention would be paid to them by the Apothecary. In general the hospital practice, in the medical departments, consists of a routine of passive medical and dietetic treatment, very different from the active and often bold practice pursued in the English and German hospitals. On looking over the prescription book of a Parisian hospital, we might suppose that the words, *saignée, sangsues, decoction blanche, eau d'orge, tisane de bourrache, de chiendent, de réglise,* designated nearly their whole materia medica.

The physicians and surgeons attached to the hospitals succeed each other every two months in their duties. Our author very properly condemns this regulation, as unfavourable at once to the interests of the sick and of the pupils who resort to these institutions for instruction. The difference in the views and doctrines of the attending physicians is often perniciously exemplified in these changes, and well calculated to fill the student with an injurious scepticism in relation to the foundations of medical science. It not unfrequently occurs, says Dr. Casper, that a phlebotomizing disciple of Broussais is succeeded by a *hœmoptophobe*, who declaims violently against the diagnosis of an obscure inflammation, and hurries on to a vigorous diaphoretic treatment, overwhelming his patient with valerian, angelica, &c.

The pupils (*eléves*) immediately attached to the Parisian hospitals, of which there are two classes, the (*eléves externes*) out door pupils, and the (*eléves internes*) resident pupils, are chosen in the following manner: In November of every year the *Administration géneral des Hôpitaux* hold a "*concurs,*"

to which all matriculated students between the ages of 18 and 24 are admitted. A sworn committee of two hospital physicians and three surgeons, chosen by lot, act as umpires in these probatory exercises of the students, and nominate from among those who have acquitted themselves successfully, the *eléves externes*, wanted to fill the vacancies in the different hospitals. In these examinations (*concurs*) the students receive by lot certain *theses* or propositions in anatomy, physiology and surgery, for extemporaneous, as well as others on the same branches, for written answers. Eighty are chosen annually; these are distributed among the hospitals in the proportion of *four eléves* to every 40—50 surgical patients, and *two* for every 60—70 patients in the *medical* wards. These pupils do not reside in the hospitals, and receive no pecuniary remuneration whatever. Their duty is to accompany the attending physicians and surgeons in their morning visits round the wards, to visit the patients again in the evening; to bleed, cup, leech, dress blisters, apply bandages and other dressings, &c. &c. The peculiar advantages of an *eléve externe*, besides the opportunities of practical instruction which he enjoys, consist in his being permitted, after having served one year, to "*concur*" again for the situation of an *eléve interne;* a post which is much sought after on account of its scientific and pecuniary advantages. For this purpose, a second "*concurs*" or examination is annually held, for determining which of the *externes* are to fill the vacant situations of the *internes* or resident pupils. Twenty of these are chosen every year, and retain their places for four years. The duties of a resident or house pupil are considerable. He has the immediate care of from 40 to 50 patients—superintends the prescriptions of the attending physician—keeps the records—collects observations—reports to the prescribing physician the condition of the sick on the preceding evening—is concerned in the (*bureau de reception*) the reception of new patients—prescribes himself in urgent cases—makes the dissections, &c. &c. One or two *internes* remain

day and night in the *chambre de garde*, an apartment kept open in the large hospitals for accidents. These pupils live in the hospitals—get their boarding, and receive, besides, several hundred francs yearly.

We shall now proceed with our author, to notice the principal Parisian hospitals. The *Hospital St. Louis* is one of the most remarkable and interesting of these institutions to the physician. It is almost exclusively devoted to patients affected with exanthematic diseases; and contains 1,100 beds.

Alibert, who stands among the first physicians of the present age, is at the head of this hospital. It is in this extensive receptacle of disease that he has collected those observations which form the materials of his splendid work on *cutaneous diseases*. Here the whole family of this loathsome class of maladies is assembled. From 900 to 1000 patients of this kind are constantly found in this institution, and in this respect, it may with propriety be called, as Dr. Casper observes, *l'égout de toutes le contrées du monde*, the common sewer of all the countries on earth. Under this head Dr. C. introduces some judicious observations on the comparative excellence of Bateman's and Alibert's classification, and, in common with the majority of his German brethren, gives a decided preference to the former.

Alibert's cardinal remedy in chronic affections of the skin is sulphur. He employs it in almost every instance in some form or other. The use of baths, too, is carried to an almost incredible extent in this hospital. The bathing establishment is calculated to accommodate patients *living without* the hospital, as well as those who are its inmates. One hundred and fifty thousand baths are said to be allowed annually to patients living in the city;* and besides these, the number of

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* As an example of the spirit of speculation among the Parisians, Dr. C. observes, that he saw over a small tavern opposite the hospital the following sign: *Ici on loue des lits aux personnes qui prennent des bains dans l'hôpital!*

patients treated in the hospital, averages 9000 a year, nearly all of which take at least one bath daily.

The sulphur vapour-bath, introduced by *D. Galés* is now almost entirely confined to the cure of itch. Biett, one of the physicians of this hospital is fond of using mercurial fumigations in inveterate syphilis. The Russian vapour-baths, too, are much employed by this physician in "rheumato-arthritic affections."

Purgatives also are a favourite remedy in this institution; and Alibert speaks in the highest terms of the utility of the extracts of *fumaria, trifolium, taraxacum,* &c. "What is more peculiar, however, in the practice of this physician, is his frequent employment of a strongly, external stimulating treatment in cutaneous affections; and this is the more remarkable, as the English, for the most part, advise a contrary treatment. In almost every lecture, Alibert says, "we must excite a local fever." With this view he directs his patients to be bathed with a solution of the nitrate of silver; and in very obstinate herpetic affections he often applies an arsenical paste. Frictions of the affected part, while in the bath, with a flesh brush, is also a frequent prescription in this hospital. The cure is almost invariably ended, in Alibert's patients, by the use of an opiated white precipitate ointment *(pommade mercurielle opiacée.)*

Alibert thinks, that when patients die of chronic cutaneous diseases, it is in consequence of the mucous membrane of the bowels becoming inflamed, from the sympathy which subsists between this membrane and the external surface, and by which a colliquative diarrhœa is produced.

Our author here introduces short sketches of the characters of Alibert, Biett, and Richerand. Alibert is not above forty years of age. In his delivery, he is off-hand, rhetorical, rapid, lively, witty, and more diverting and interesting than instructive. In his exterior, as well as in his conversation, he is altogether the Frenchman. He recommends, in the same breath, the study of the Arabians, and a new work on physi-

ology—produces a patient and a drawing from his splendid work—throws the prospectus of a new work among the audience, and distributes a sample of a new refreshing beverage among the pupils; and whilst they are tasting around, spices his already too piquant discourse, with witty anecdotes, and *bons-mots*.

Biett is a young man, of great merit. "If it be pleasing," says Dr. Casper "to eulogize what is praise-worthy, I enjoy this pleasure in the most lively manner in speaking of this interesting and ingenious young man." Not one of his colleagues is so deeply interesting and instructive in their hospital visits as Biett. He is extremely collected, earnest and shrewd in his clinical examinations, and, by consequence, generally very accurate in his diagnosis and curative indications. He is obliging in his manners, fond of discussion, and zealously devoted to his profession.

Hotel Dieu. This hospital is situated in the centre of the most populous, noisy, close, and dark quarters of Paris. It contains twenty-three large halls. It has 1300 standing beds, with a considerable number of *lits suplémentaire*, for occasions of emergency. All classes of diseases, except the diseases of children, syphilis, itch, and mental maladies, are received into this institution. *Dupuytren* is at the head of this hospital. He is undoubtedly the first surgeon in France. His clinical lectures are truly interesting and instructive. He is not above thirty-seven years of age, and possessing an acute and vigorous understanding, great industry and zeal in the pursuit of his profession, and a most extensive field for experience, we have reason to hope important accessions to surgery from the future course of this active and intelligent surgeon. His delivery is graceful, and his language strong and perspicuous.

There is, however, one trait in his character, which is by no means an amiable one. In his deportment towards his patients and his pupils, he is often extremely unkind, cold, and peevish.

Having given a sketch of the character of this eminent surgeon, our author next describes the manner in which he goes through his hospital duties. After having been round the wards, he repairs to the *operation-hall*, followed by a great number of *eléves externes* and *internes*, and a large crowd of other attendants at the hospital. Here he delivers instructive remarks on the new and interesting cases that may occur in his wards, gives the results of autopsic examinations, exhibits specimens of morbid anatomy, and finally proceeds to perform such operations as may be required at the time. Dupuytren operates with uncommon neatness, despatch, and certainty.

The author here introduces short accounts of some of the principal operations which he witnessed by this surgeon.

In a case of polypus of the nose, of the fibrous kind, the extraction of which had been several times unsuccessfully attempted, Dupuytren, having first introduced an elastic sound through the nose into the fauces, and fastened to it a dossel of lint, slit up the wing of the nose on the affected side, (according to the old plan,) and then seized the polypus with a forceps, and extracted it completely.

The author states that Dupuytren performed the operation of lithotomy *five times* in the course of two years, for the purpose of extracting broken-off-ends of catheters that had slipped into the bladder. He has extirpated twelve cancerous uteri. In two instances only, however, did the patients obtain complete relief. In one case the patient died under the operation.

The *physicians* belonging to this extensive hospital, are Recammier, Husson, Petit and Montaigne. They are all more or less attached to the doctrines of Broussais; Husson especially is a warm partizan of Broussais. With him, consequently, every thing is *gastro-enteritis*, and leeches are a principal means of treatment. Recammier, who has the appearance of an old and calm man, is in reality a lively, imaginative, and in some respects an original spirit. He possesses much learning, and is esteemed one of the first physicians

in Paris. In accuracy of diagnosis there are few who are his equals.

La Charité. This hospital contains but 800 beds, 104 of which are appropriated to surgical patients. It is, notwithstanding, a very interesting clinical institution, on account of the eminence of several of its teachers. *La Charité* is remarkable for the number of pulmonary diseases, and of *colica pictonum* which are constantly brought to it. Consumption, indeed, appears to be an exceedingly common disease in Paris. M. de Chateauneuf, in a statistical paper presented to the French Academy in 1820, has shown that out of 64,441 deaths in Paris, during the years 1816, 1817, 1818, the number and proportion of pectoral diseases were as follows: 604 asthma, 1894 inflammation of the lungs, 4259 catarrh, 6971 phthisis pulmonalis. According to this statement, therefore, the proportion of deaths from asthma was nearly as 1 to 100, of pneumonia as 1 to 83, of catarrh as 1 to 15, and of consumption as 1 to 9.

The treatment of pectoral diseases in *La Charité* is exceedingly simple and inactive. Great attention is paid to the appearances and character of the *sputa*, and the thorax is examined according to Laennec's method with the utmost care; but the therapeutic management is feeble and temporizing; consisting often of little more than the exhibition of pectoral syrups;* ptisans, and occasionally a little *kermes*. Indeed the whole medical treatment in *La Charité* consists of a *stereotype* routine, which has been pursued from time immemorial with the most pertinacious exactness.

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* Of these the following are the most frequently employed in this institution.

Sirop pectoral de Chaussier.

℞ Kali sulphurati	- - -	℥jss.
Sacch albi	- - -	℥x.
Aq. Hysop	- - -	℥vj.

f. l. a syr. a tea-spoonful mornings and evenings in chronic-inflammatory pectoral affections.

The medical gentlemen attached to this hospital are *Boyer*, *Roux*, *Lerminier* and *Fouquier*. Boyer is already considerably advanced in years; and on account of his infirm state of health, is not often seen at the hospital. As a writer and practical surgeon, Boyer is deservedly placed in the first rank of French surgeons. As a public teacher, however, his merits are far from being great. Both in his conversations at the sick bed, and in his public lectures, he is intolerably monotonous and dull. *Roux*, his son-in-law, is a young man of very considerable learning and talents. He possesses an enterprizing, prompt, and lively disposition, and is ardently devoted to his profession. He is very popular as a teacher. *Lerminier* has charge of the principal wards of *La Charité*. In his treatment of diseases he is a most inveterate *routinist*. With a small number of ptisans, and *magistral-formulæ*, he generally pursues a mere temporizing palliative course, and seldom directs his diagnosis beyond the more obvious phenomena of diseases. He is kind and familiar towards his pupils, but his lectures are for the most part little instructive. *Fouquier* is one of the most esteemed Parisian physicians; few of his countrymen equal him in the accuracy of his diagnosis. He examines with great care, before the pupils, such cases as come under his notice in the hospital, as well as the reports which his pupils are obliged to draw up of particular cases, and criticises them with earnestness and severity, where severity is in place. Fouquier's practice is peculiarly remarkable for the enormous doses of powerful medicines which he pre-

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Sirop anti-asthmatique.

℞ Ammoniaci depurat - - ℥jj.
Macer cum, vini gallici alb. - ℥vjjj.
adde
Sacch - - - - libj.

f. l. a syr. Dose from ʒj. to ℥j, three or four times a day.

Pilules anti-catarrhales de Pariset.

℞ Tart. antimonii; extract. opii. aa. gr. jjj. G. Tragac. gr. x. Cons. rosar. q. s. f. l. a. Pill. No. 60. S. two mornings, and two evenings, in chronic catarrhs.

scribes, as of opium, conium, nux-vomica, sugar of lead, &c. As a teacher of clinics, Fouquier is unquestionably the most interesting and instructive of all his colleagues.

We pass over our author's accounts of the hospitals, *des Veneriens, des Enfans Malade, de la Pitié, Necker, Beaujon, Cochin, du Val-de-Grace, and Hopital Militaire de la Garde Royale.* In the *hopital St. Antonine,* the following remedy is much used, and with considerable success, in chronic, mucous, and sanguineous discharges from the bowels.

℞ Rad. Ratan. - - ℥j.
Spir. Vini alcohol - ℥jv.
f. l. a. Tinct. S. gtt. xjj. to ℥j. daily.

Beauchesne, principal surgeon of this hospital, has lately made some interesting cures of cancer without an operation. Our author mentions a case of this kind, in which he had an opportunity of witnessing this physician's mode of treatment, and its effects on the disease. A woman aged sixty-five, affected with cancer of the nose, was received into the hospital. The disease had commenced two years before; the right wing of the nose, and the whole right side of the upper lip, was deeply ulcerated, being attended with excessive pain. The treatment was as follows: The ulcer was daily washed with injections of a decoction of cicuta, in which corrosive sublimate was dissolved in the proportion of two grains to ten ounces of the decoction. In addition to this, the patient took large doses of cicuta internally with small doses of corrosive sublimate. After four month's continuance under this treatment, the lip was entirely healed, except a point about the bigness of a pin's head at the angle of the lip.

The fifth chapter of this work is devoted to a particular account of the practice pursued in the Parisian hospitals, in the treatment of certain important diseases.

Epilepsy.—The hospitals *Salpetriére* and *Bicêtre* admit more epileptic patients than all others put together. A large proportion of these patients are affected, at the same time, with mental disease; and are received more for the purpose of

affording them an asylum than with a hope of curing them. Out of 250 epileptics, which our author saw in the hospital *Bicêtre*, Pariset selected but nineteen as fit subjects for remediate treatment. This physician is very fond of employing the *actual cautery* in epilepsy. He applies it upon the vertex of the head; and thinks that he has done more good with it than with any other remedy. Esquirol, also, in the hospital Salpetriére, has employed the actual cautery in this way, with some advantage. He puts no confidence whatever in the employment of the metallic oxydes in this disease.

Biett, one of the physicians of *St. Louis*, on the contrary, considers the *nitrate of silver* as an important remedy in this affection, and is said to have employed it in several instances with complete success. Dr. Casper saw an epileptic in this hospital, who had taken eight grains of nitrate of silver daily, for two years; his skin had acquired a dark colour, like that of an American Indian. The paroxysms which at first returned daily with great violence, became less severe, and recurred only every month. Besides this remedy, Biett is fond of giving the essential oil of valerian, to patients labouring under this disease, in doses of from forty to fifty drops. He also employs *spir. terebinth* in drachm doses; and the *animal oil* of Dippel, to which latter article he is particularly partial; and which he administers in doses of 150 drops twice daily.

Itch.—The root of the *plumbago Europea*, which was introduced into regular practice by Sumeire, is frequently employed both in private and in hospital practice, and is by many regarded as among the most efficacious remedies in this disgusting disease. It is employed in the following way: Take three handfuls of the root, and one of common salt; beat them well together in a mortar, then pour on it one pint of hot sweet oil. Strain off the oil and tie up the residuum in a little linen bag. In using it, the bag must be dipped in the warm oil, and with it, the whole surface of the body rubbed every morning and evening until the disease has dis-

appeared. Three or four frictions are said to be sufficient in ordinary cases. The *aunus nigra baccifera* is used in the same way; and *Percy* asserts, that he has never known it to require more than ten days to cure the itch with this article.

Alibert is much in the habit of employing an ointment composed of two parts of *flor. sulphuris*, one of carbonate of potash, and eight of lard. Frictions with this ointment are made three times daily. *Dupuytren*, in his practice in the Hotel-Dieu, employs a wash made by dissolving four ounces of kali sulphuretum in three half pints of water, and adding half an ounce of sulphuric acid. The patient is directed to wash himself with this. *Chaussier* recommends his *savon mercuriel*; it consists of a combination of three ounces and a-half of mercurial ointment, and three of a caustic solution of soda. This compound is said to be very efficacious in other hepetic eruptions, and in different species of psora.

Arsenic, though not a new remedy in chronic diseases of the skin, is, perhaps, no where so much employed in inveterate cases of this kind, as in the hospital *St. Louis.* Biett, especially, places much confidence in its powers. In that species of herpes *(herpes exedens)* which sometimes attacks the nose of scrophulous persons, *Alibert* invariably applies frére Cosme's* paste to the ulcer.

It is however chiefly as an internal remedy that arsenic is extensively employed in this hospital. *Biett* prefers, for this purpose, the *arseniate* of *ammonia.* This preparation is made by dissolving one part of arsenic in four parts of nitric, and a-half of muriatic acid. The solution is then saturated with carbonate of ammonia, and evaporated. The product thus obtained is the arseniate of ammonia. One grain of this salt dissolved in an ounce of distilled water, with the addition of

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* It is thus made: Take two drachms of cinnaber, eight grains of the ashes of burnt shoe soles, twelve grains of dragon's blood, and forty grains of white arsenic: reduce them to powder, and make it into a thin paste with water.

a little spir. lavend. is the usual form for exhibiting this remedy. The dose is from 25 drops to a drachm daily. The *Asiatic pills*, also, the formula of which was first published in the Asiatic Researches, are frequently employed in this hospital.* In this place the author introduces short notices of several inveterate cutaneous affections which were cured with this remedy, during his attendance at the hospital. A man aged forty, who had been affected for several years with a severe herpetic eruption of the scalp, and in whom the disease had already given rise to occasional symptoms of mental derangement, was perfectly cured in three months by the above solution. He saw, also, two cases of *herpes furfuraceous*, cured with this remedy in a short time. In one of these cases but three grains of arsenic, and in the other but five were used. A case of impetigo *(dartre crustacée humide)* resisted this remedy for a long time; finally, however, it yielded to its powers.

Tinea.—The treatment of Tinea in the Parisian hospitals, consists almost entirely in the use of *an arcanum!* Two brothers of the name of *Mahon* are in possession of a secret remedy, which, by an order of government, is employed under their immediate direction, in all the hospitals. For this service they are allowed one thousand francs yearly, and six francs besides for every patient cured. Of 795 children, 527 were, in the space of five years, radically cured of tinea by this *arcanum.* This remedy consists of a powder and an ointment which our author thinks contains *carbonate of soda.*

Alibert's external remedies in tinea, consist almost exclusively of ointments composed of lard and pot-ash,—or of lard and powdered charcoal,—or finally, of lard, pot-ash, and charcoal. Internally, he frequently directs emetics and pur-

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* These pills consist of one-seventh of a grain of arsenic, and six-sevenths of a grain of black pepper. One grain of this mass is taken at a dose.

gatives; the expressed juice of *trifol. fibrinum, borrago,* decoct. bardanæ, flowers of sulphur, crocus martis.

In England, the oil of turpentine has been lately employed in tinea, with considerable success. The head is bathed with a mixture of one ounce of spir. terebinth, and two drachms of ol. olivar.

Syphilis.—In the practice of the Parisian hospitals, as every where else, mercury constitutes the principal remedy in the treatment of this disease. A solution of corrosive sublimate in alcohol, is the usual form for exhibiting this remedy. In addition, however, to the employment of this active mineral, *diaphoretic ptisans* are very commonly administered in this disease. The "*Tisane de Felz*" is a favourite prescription in the hospital *St. Louis.* It is made according to this formula.

Stibii sulphurati nigri	-	-	-	℥xj
Rad. Sarsaparill.				
Chinæ	-	-	-	āā ℥vj
Herb. Hederæ ter				
Ichthyocollæ	-	-	-	āā ℥ss
Coq. cum aquæ lib. iv ad Col. lib. jj adde				
Hydr. muriat. corros.	-	-	-	gr. j
S. M. To be taken in the course of 24 hours.				

Gold, according to our author's account, does not sustain the reputation it had acquired as a remedy in this disease. It is still, however, employed by some Parisian physicians, in inveterate cases of this kind, especially where mercury has failed to perform a cure.

Cullerier has made some experiments with the *muriate of platina,* in syphilis.* He selected seven patients for this purpose. One of these had small ulcers on the glands penis, which could not be certainly pronounced syphilitic. The *muriate of platina* was prescribed, of which he took four and a third grains in the course of twelve days, by which

* Dict. des Sciences Med. Art. Platine, 1820.

time all the symptoms were gone. A second patient had phymosis and bubo. He took five and a half grains of this preparation in the course of five weeks, and was cured. A third patient, who had venereal excrescences, received no benefit from this article, although he took ten grains. A fourth one was affected with painful exostoses; he took ten and a half grains, by which he was entirely relieved from the pain; the tumours, however, were but slightly diminished. A fifth, had gonorrhœa, chancre, and bubo. He took the platina ten weeks. In eight weeks the chancres and bubo were healed. A sixth patient had syphilitic eruptions. He took nineteen grains, but was only partially freed from his disease. The seventh case was similar to the preceding one; no radical cure was effected in this instance by the platina.

Injections are not commonly employed for the cure of gonorrhœa, in the French hospitals; and in the *hôpital aux veneriens*, primary chancres are seldom touched with caustic. In the deep secondary ulcerations of this disease, however, Cullerier applies the butter of antimony; and in the hospital *St. Louis*, such ulcers are cauterized with nitric acid.

We pass over our author's description of Laennec's stethoscope, and proceed to his account of Fouquier's "*mode of employing sugar of lead in pulmonary consumption.*" Sugar of lead is an old remedy in this disease.* But the great extent to which its internal use may be carried, without producing deleterious effects in the system, is not very generally known. According to the experience of Fouquier, *sugar of lead* possesses a *specific* power against the colliquative sweats of phthisis. He denies that it can produce colica pictonum; an assertion which we are, however, by no means disposed to receive as correct. We have

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* In the Pharmacopœia Bateana, published in 1713, there is a formula for a *Tinctura Antiphthisica*, which consists of a solution of sugar of lead and *sal-martis*. It is stated "to be truly a good medicament in those consumptions which proceed from ulcers of the lungs."

ourselves seen several cases of lead cholic, which were, unequivocally, produced by this preparation. It is nevertheless certain, that it is much less apt to give rise to this affection than is generally supposed. Fouquier asserts that it may be given to the extent of twelve grains daily, without danger. The medium portion *per diem*, is from four to eight grains. In employing it in consumption, its use should be continued as long as the colliquative sweats return. Dr. Casper saw thirteen cases treated with this medicine in *La Charité*. Four of these were cured, five died, and four were much relieved. Dr. C. here introduces an account of several cases of this disease, in which the beneficial effects of this preparation were conspicuously demonstrated.

"A man aged forty-seven, far advanced in phthisis, and greatly exhausted by excessive colliquative night sweats, was put on the use of the sugar of lead, on the 20th of September. He took one grain daily, until the 24th, by which time the sweats were considerably lessened. He continued to take this quantity daily, until the 29th, when he was entirely freed from his exhausting night sweats. On the 4th of October, however, the sweats returned. He now took six grains daily, until the 10th, when they were again completely suspended; this dose was nevertheless still continued, until the 16th, when it was increased to eight grains, daily. There were no night sweats. On the 18th, moderate perspiration in the morning; on the 19th, no sweat. The last dose was continued. On the 20th, the daily dose increased to eleven grains. No sweat. By the end of the month the patient was well, and left the hospital.

"A woman aged thirty-three, who had unequivocal symptoms of phthisis, and who suffered, especially, from exhausting night sweats, took, on the 27th of August, three grains of sugar of lead. On the 29th, she took seven grains, which had the effect of diminishing considerably the colliquative sweats. On the 30th and 31st, she took eight grains: the sweats, however, again increased. September 1st, took nine grains; sweat diminished. On the 2d, took ten grains, which put an entire stop to the sweats. The medicine was continued a few days longer; and on the 15th, left the hospital entirely relieved from this distressing symptom."

"A young woman affected, for eighteen months, with a catarrhal affection, attended with profuse and debilitated night sweats, was put on the use of the sugar of lead, on the 18th of August. After she had gradually increased the dose to nine grains, daily, the night sweats were considerably lessened. On the 26th, no sweats. She now became affected with violent colic, attended with diarrhœa. The medicine was discontinued for a

few days, but as the night sweats again appeared, she took, on the 12th of September, four grains of the lead. 13th, took six grains, no colic, and but little sweat during the night. 16th, dose increased to seven grains, which was continued. The sweats returned no more; and the patient was discharged by the end of the month."

The author goes on to relate the circumstances of other similar cases; all of which undoubtedly go to show that *sugar of lead* is a valuable remedy against this one symptom,—*colliquative night sweats.* The author does not say, however, whether, in other respects, there was any improvement effected in the disease. The mere removal of this symptom will no doubt make the patient much more comfortable, and economize his strength; yet we cannot presume that it would strike at the root of the disease,—that it would, in fact, cure the local pulmonic affection.

Colica Pictonum.—We never find the hospitals *La Charité* and *Hotel Dieu*, without a considerable number of patients affected with this disease. The treatment pursued is a settled *routine* of *prescriptions*, which are punctiliously adhered to in these institutions, and which is familiarly known by the title of *traitement de la charité.* When a person labouring under this disease, is admitted into any of the hospitals, the first prescription is the (*lavement purgatif des peintres*) the painter's purgative enema, viz:

"℞

Folior. Sennæ	- -	℥ss
Coq. c. aq. comm.	- -	℥xii Col. vjjj
adde		
Sodæ Sulphat.	- -	℥ss
Vin. Antimonii	- -	℥iv

S. M. To be used as an enema.

"During the day the patient must drink the *eau de casse avec les grains*, namely:

"℞

Cassiæ fistulæ	- - -	℔j
Coq. c. aq. comm. l. ad Col.		℔jj
Tart. natronat.	- -	℥j
——Antimon.	- - -	gr. iii

S. M.

"In the evening the (*lavement anodin,*) anodyne enema is administered, made of nut oil, ℥iv and red wine ℥xii. Three hours after this, three drachms of theriac, with a grain and a half of opium, are given.

"On the following day, the patient gets the *eau benite*, according to the following formula:

"℞

Tart. antimon.	- - -	gr. vi
Aq. destil.	- - - -	℥viii

M. The half to be taken directly, and the remainder in an hour after.

"If this produce vomiting, the patient is put on the use of the following ptisan:

"℞

Liq. Guaiaci		
——Sassafras		
Rad. Chinæc		
——Sarsaparilla	- -	āā ℥j
——Liqueritiæ	- -	℥ss

Coq. l. a. c. aq. lib. jjj ad col. lib. jj

"In the evening, the *lavement anodin* is repeated. On the third day the patient drinks two pints of the above ptisan, in which one ounce of Fol. Sennæ is previously boiled. In the evening the first enema (*des peintres*) is repeated, which is followed in two hours by the second enema (*l anodin*) and the dose of theriac and opium. On the fourth day, the patient commences with a new medicine,—*purgatif des peintres*, viz:

"℞

Fol. Sennæ	- - -	℥jj
inf. c. aq. q. s. ad colat.	-	℥vi
Soda Sulphat.	- - -	℥ss
Rad. jalap	- - -	℥j
Syr. Spinæ Cervin.	- -	℥j

S. M. During the day, the above sudorific ptisan.

"On the fifth day, the *tisane sudorifique laxative* is continued, and in the evening, the first enema, followed by the second anodyne one, with the dose of theriac and opium are administered.

"On the sixth day, the above *purgatif des peintres;* the simple sudorific ptisan,—then, in the evening, the two enemata, with their consequent dose of theriac and opium. Commonly the disease disappears after the second purgative; should the pain, however, continue, the strong purgatives are to be exhibited, until the disease yields. If, notwithstanding, the bowels remain too constipated, and the patient do not vomit and purge, he must then take the following active cathartic:—(*bols purgatifs des peintres.*)

"℞
Aloes soccot
Extr. Rhæi - - - āā gr. x
Jalap
Hellebor. nig.
Scammon - - āā gr. xiv
Syr. Spinæ Cervin. q. s. x. boli No. vi. one every three hours."

This, then, is the fixed *stereotype* course pursued in the treatment of colica pictonum in the Parisian hospitals. PINEL very justly calls this practice an *empirisme consacré par une longue suite de succés;* and it must be confessed, observes our author, that the success attending this treatment has not been small, since, out of 1353 patients, treated in this way, in *La Charité,* during twelve years, only 64 died. P. 258.

We cut short our review of this interesting work, in this place, reserving the remainder of the volume for notice in our next number. E.

ART. IV.—*Principles of Medical Science and Practice.* Part I. *Physiology.* By HARDWICKE SHUTE, M. D. Physician to the General Infirmary, and to the county and city Lunatic Asylum, Gloucester. Octavo, pp. 566. London, 1824.

IT was not said without foundation, that "of making books there is no end, and much study is a weariness of the flesh." In our occupation we are often obliged to wade through some half score of new publications, before we can bring together as many interesting facts, or even opinions, as may serve for the commencement of a review. It seems to have become the fashion of this book-making age, to write with prolixity instead of brevity or perspicuity. An author catches up a new idea, and before it gets out of his hands, it becomes enveloped in a whole octavo of every day matter. No where is this more generally the case, than in our own mother country, of whose liveliest author it was once observed, that "he could

lecture all day upon a broom-stick." But the physicians and surgeons of Britain, especially, appear to have acquired the habit of which we are forced to complain. Many of their publications are made to cover pages altogether beyond the proportion of ideas which they convey; and the size as well as expense of new medical books has, of course, become too exorbitant for the general class of readers.

It was with a view to these very circumstances, that we were first induced to undertake the charge of this journal; inasmuch as by condensing the current knowledge of the day within some reasonable limits, we hoped to bring every thing of a novel or interesting character before the eyes of our readers. The work now under review, however, has proved too serious an impediment to be leaped over by the most zealous efforts. We have laboured harder and longer to give it what is usually called a full reading, than we have generally been obliged to do upon any single publication; and now that we have examined it from beginning to end, we find it difficult to revert to any part from which our pages can derive much interest. The author is a very sciolist, who often quotes names and opinions for authority, instead of resorting to facts or to reason. He is evidently a closet philosopher, who undertakes to establish his own views by mere speculation, while he boldly assails the opinions of such writers as Philip and Le Gallois, without adducing the authority of a single new experiment. Still, however, even an author of this character may occasionally throw out ingenious ideas respecting circumstances which others would have passed over with less examination. To do justice to Dr. Shute, we must confess that we have found this to be the case in several parts of his book; and from the turn of mind which he evinces for elaborate discussion, we have no doubt that he will appear to much greater advantage in the practical part, with which we suppose the second volume will be occupied. In another respect, also, we feel constrained to allow no small degree of credit to our author. He has brought together ma-

ny interesting facts and opinions, which in a detached situation would have excited no particular notice; and in some places he supports his peculiar views with considerable force and ingenuity. We will undertake, therefore, to review the book, notwithstanding the unfavourable opinion we have expressed of its general merits, and of the unreasonable size to which it has been extended.

The great object of our author, as far as we can discover it in the preliminary essay to this physiological part of his work, is to prove, that all the functions of the body are of a *digestive* character. The introduction of food into the stomach is not for "its conversion into chyle, or even into blood; it is intended to constitute a component part of the animal frame." He thinks, therefore, that "the conversion of certain parts of the *blood* into the component parts of the body, is accomplished by a process similar to that by which certain parts of our food are converted into chyle,"—in other words, that it is digestion; and that the process of digestion is not accomplished until "certain parts of our food are converted into the component parts of the animal frame." He intends to show that "the different functions of digestion, absorption, assimilation, circulation, nutrition, secretion, and excretion, all constitute one extended system of actions, by which our food is in reality digested." He thinks that the removal of those parts from the system which are not calculated to enter into its composition, is also effected by a digestive process; and upon these grounds he has "ventured to adopt the unusual expression, that the blood is digested." Respiration he also makes out to be a digestive process, by means of which the "ærial influence" is digested into the "arterial influence," which in its turn becomes afterwards digested into the "nervous influence." The air, like the food, he therefore supposes to enter into the body as a component part of its organization. "If the ærial influence cease to constitute a component part of the organization," he conceives "that that organization ceases to be endowed with the principle of life, and

therefore ceases to be endowed with the susceptibility of action, &c."

In relation to these propositions, it may be observed, that they do not differ essentially from the opinions commonly entertained by physiologists. The only important difference consists in the application of the term digestion, to which our author has allowed a much more extended sense than has been hitherto accepted by the profession. It may be questioned, however, whether such a latitude in the use of terms is calculated to produce any good effect in scientific discussions. We have always found it best to call things by their right names; and cannot approve, therefore, of this grouping together several different processes of the animal economy under one appellation, merely because they happen to resemble each other in some imaginary shades of character.

The body of the work is comprised in eighteen chapters, the subjects of which are as follows:—1. *Digestion*, (properly so called)—2. *Absorption* —3. *Assimilation.*—4. *Respiration.*—5. *Circulation.*—6. *Nutrition.*—7. *Secretion.*—8. *Excretion.*—9. *Electricity.*—10. *Nervous Influence.*—11. *Living Principle.*—12. *Living Action.*—13. *Locomotive Functions.*—14. *Digestive Functions*, (Respiration)—15. *Digestive Functions*, (Circulation!) 16. *Digestive Functions*, (Nervous action!) 17. *Digestive Functions*, (Animal heat!) 18. *Sensitive Functions.*

It is in only a few of these chapters that any novel or very interesting matter is contained. Although we shall, therefore, run them over hastily, we will not, however, neglect to detail, as far as possible, every thing that can instruct our readers.

Under the head of digestion, we have not been struck with any thing of a peculiar character, except, perhaps, the opinion that the formation of chyle is completed in the stomach. He grounds this opinion chiefly on the fact, that Mr. Hunter once detected a *whitish fluid like cream* above the pylorus in some dogs, upon which he was experimenting for the pur-

pose of ascertaining whether the gastric fluid was of an acid or alkaline nature. After controverting Sir Everard Home's statement respecting the hour-glass contraction of the stomach, on the authority of "several experimentalists," whom, by the way, he does not name,—he proceeds as follows:

"There is nothing in the opinions detailed, which does not appear to justify the conclusion, that the food, in consequence of its admixture with the gastric fluid, and of the living action of the stomach, is separated into certain elementary particles, which are capable of re-uniting in those peculiar proportions which constitute chyle. The food thus changed by the action of the stomach, consists, as before stated, of certain particles united, or capable of uniting, in those proportions which constitute chyle, and of others which have either not been decomposed, or are not capable of re-uniting in the peculiar proportions mentioned. Dr. Hunter proved by experiment, that the food is *animalized* in the stomach. Some bread, which had been allowed to remain in the stomach of a dog for some hours, did not pass into the acetous fermentation, but became *putrid*. This peculiar change belongs to animal matter alone; and if that important change by which vegetable becomes animal matter, takes place in the stomach, any secondary process appears to be unnecessary; and therefore I conceive that the *chyle* is formed by the action of that organ."

What is the character of the above mentioned "several experimentalists," who could not discover any hour-glass contraction of the stomach, we cannot venture to conjecture; but we can assert that we have repeatedly seen that phenomenon exactly as it has been described by Sir E. Home, although we do not pretend to decide that it must always occur during the process of digestion. Of the truth of the facts quoted from the two Hunter's, we are not disposed to doubt. But they appear to us to be susceptible of a different explanation from that which the author has drawn from them. The kind of food with which the dogs were fed in Mr. John Hunter's experiments, is not specified; and the white fluid, by which his own curiosity does not appear to have been much excited, may have arisen from an admixture of milk with the food. No other physiologist has ever detected chyle in the stomach; and the probability is, that Mr. Hunter himself was not perfect-

ly satisfied with the opinion which our author advances in relation to this subject. That most careful observer would never have stated the circumstance with so few accompanying remarks, if he had intended to draw any positive conclusions respecting it. The results of Dr. William Hunter's observations upon the putrefactive disposition of the chyme, are also liable to equally strong objections. He allowed some bread to remain eight hours in the stomach of a dog, after which the animal juices which it had absorbed, putrified on exposure to the air. The vegetable matter might still have remained only partly changed, and its acetous fermentation have been disguised by the offensive products of the putrefaction of the gastric secretions. At all events, it is certain that the acetous fermentation frequently takes place in healthy stomachs; and it is now equally well known, that the sensible qualities of food are not always destroyed by the full action of the digestive liquor. Magendie declares that "there are as many kinds of chyme as there are sorts of aliments," and this opinion has been confirmed by numerous observations in man and in animals. We cannot agree with our author, therefore, in his opinion that the formation of chyle is completed in the stomach, and that the bile and pancreatic juice are of no other service than to stimulate the intestines to their natural peristaltic contractions.

In his chapter on absorption, he devotes three pages to the serious refutation of an idea, which we do not know that any body was ever silly enough to entertain,—that the oxygen of respiration gets admission into the blood through the medium of the absorbents. After discussing this point at some length, he very cunningly concludes, that as

> "The lymphatics pour their contents into the venous blood *before it arrives at the lungs*, that change, therefore, in the appearance of the blood, which we attribute to the absorption of oxygen, ought, consistently with such an idea, to take place before, and not after, its passage through that organ; but the fact we know to be otherwise; and therefore we presume that the absorbents are not at all concerned in that action by which, in the process of respiration, the oxygen of the air unites with the blood."

But we will not criticise the laughable gravity of this kind of reasoning, lest we should be suspected of entertaining some jealousy at his dignifying himself with our prerogative right to the plural number.

A little further on he solves the knotty question, respecting cutaneous absorption, by the following right handy conclusion. "If the cutaneous absorbents do not act upon the surrounding atmosphere, there is no reason to suppose that, in a state of health, they act upon any thing which is exterior to the body itself." Respecting the origin and use of the lymphatics, he despatches, at one blow, the doctrine of Humpage, the opinion of Mascagni, and the researches of Brodie and Magendie. They are "generally considered to be erroneous," —they have become "almost obliterated by time," and he thinks it "wholly unnecessary to revive opinions which have died a natural death!"—But who, let us ask, has refuted the assertion of Mascagni, in regard to the connexion between the arteries and lymphatics; or who has overthrown the conclusions of the French physiologist, concerning venous absorption? It surely is idle to declaim without proof; and it is equally unreasonable to reject positive evidence because it happens to be opposed by more fashionable doctrines.

Under the head of Respiration, after having quoted from Dr. Henry an account of the chemical changes in the composition of the atmospheric air, he observes that,

"The process of digestion is here satisfactorily described. The principles of the air are *disunited;* the oxygen, that is, the nutritious part of the air, is lost in inspiration; and nitrogen, its excrementitious part, is separated from it, and eliminated from the system by expiration. I shall have occasion, in a future part of these observations, to advert to the generally received opinion, that the oxygen lost is contained in the carbonic acid expired; but I cannot enter upon this part of my subject at present, because it is intimately connected with the opinions I entertain of living action in general, which opinions have not yet been presented to the consideration of my reader. I shall, therefore, for the present, assume the fact that the oxygen contained in the carbonic acid expired, is *not* the individual oxygen which was conveyed to the lungs in the previous inspiration, that is, the inspiration immediately antecedent to its evolution from the sys-

tem in the act of expiration. That the oxygen of the air *unites with the blood* in its passage through the lungs, appears to be admitted by the concurrent testimony of the most eminent physiologists."

To prove that oxygen is actually introduced into the circulation, he quotes the *opinions* of Saumarez, Paris, and Cuvier; and it is really amusing to see in what manner he occasionally contrives to get the advantage of this kind of assistance. For instance, Dr. Paris, in describing Bichat's doctrine of the source of muscular irritability, happens to make use of the words, "*duly oxygenized blood.*" Our author immediately seizes hold of the sentence which contains this fortunate expression, and brings it forward as a proof in favour of his theory. But he stumbles at the fact that "we are unable, by the most delicate tests, to show that arterial contains an *iota* more of oxygen than venous blood." The following quotation will show how he endeavours to get over this difficulty, at the same time that it will elucidate his besetting idea of ærial digestion.

"Had it been proved that arterial contained more oxygen than venous blood, no difference of opinion could now exist as to the fact whether the oxygen of the air does or does not unite with the blood in its passage through the respiratory organs. We are, therefore, under the necessity of admitting, that according to the views of the most eminent physiologists, with the exception of a few individuals, the oxygen of the air does unite with the blood, but cannot subsequently be discovered in it. How can we reconcile this fact with the phenomenon observed, without allowing that the oxygen must have entered into a new combination, by which its original properties are lost. This product of respiration, without, at present, adverting to the manner in which it has been formed, may be, and has been, denominated the *arterial influence.* In chyle, the product of digestion, we cannot discover the principles which previously existed in our food; in the arterial influence, the product of respiration, we cannot discover the principles which pre-existed in the air. It appears, then, that the component parts of the air are first disunited; the oxygen, the *nutritious part*, combines with *some other substance*, in those peculiar proportions which constitute the arterial influence; that nitrogen, the excrementitious part, is separated from the air for the purpose of being eliminated from the system in the act of expiration; and that the product of these combined actions, is a substance essentially different from the atmosphere to which its origin must be attributed."

When we first encountered his idea of a "some other substance," which, by uniting with the oxygen of the air, constitutes the arterial influence, we confess that we were put to our wit's end to discover its meaning. In the concluding chapter of the book, however, we came in full contact with the very thing itself, in the shape of electricity, which he seems to imagine is given out from the lungs to the atmospheric air. The same agent, he believes, is given out from the mucous membrane of the stomach, to the various injesta, by means of which their nutritive particles are converted into sound and well digested chyle. Now, seriously, this is carrying the point a little too far. We ourselves have always experienced a lurking disposition to lay upon galvanism a sufficiently hard task to perform; but when it is told to us that, in addition to the excitation of secretion, of nervous sensibility, and of muscular contraction, the same influence digests the air, the food, the blood, the lymphatic fluid, and almost every thing else in and about the body, we must confess that our credulity is taxed to the very utmost point of endurance.

In the chapter on excretion, we meet with a well attested, and ingeniously supported fact, which is worth a whole multitude of mere opinions, such as our author has quoted to prove his doctrine of respiration. It is, that carbonic acid gas is given out from the blood through the cutaneous exhalents. We are surprised that Dr. Shute did not bring this forward in his argument respecting the oxygenization of the blood, inasmuch as we conceive that it completely establishes that theory. The carbonic acid gas of expiration can originate from only two sources,—either from a direct combination of the atmospheric oxygen, with carbon excreted from the venous blood, or from a direct exhalation of carbonic acid, by the pulmonary capillaries. Now, if this gas is actually exhaled from the whole external surface, there can be no reasonable doubt that it is exhaled also from the lungs. That this is the case, we think the following circumstances, which are detailed at full length by our author, will abundantly prove: Count de Milly once

observed, while laying in a bath, "a prodigious quantity of small silver-coloured air bubbles, resembling what are called the seeds of pearls, on the surface of his body." These he collected in a common decanter, and submitted to Lavoisier, who ascertained that they were composed of pure fixed air. Dr. Priestley, and others, afterwards attempted to explain this phenomenon, by the supposition that the water must have contained oxygen gas, which united with carbon excreted from the skin, and thus became converted into fixed air. Spallanzani and Abernethy, however, have refuted this supposition by various experiments, in which carbonic acid was produced from the surface, independent of the presence of oxygen in any shape. The results of Spallanzani's experiments are detailed in a letter to Senebier, introductory to his memoirs on respiration.

"He appears to have been completely satisfied that the air in contact with the skin, is changed exactly in the same manner as by respiration. When atmospherical air was employed, oxygen disappeared, and carbonic acid was produced; but he is of opinion, that the oxygen of the atmosphere had no share in the production of the carbonic acid gas, because it was equally great when the animal was confined in gases containing *no oxygen;* and he therefore concludes that the carbonic acid gas was exhaled, and the oxygen absorbed."

Mr. Abernethy's experiments, as described in the Ed. Med. and Surg. Journal, for 1805, and quoted from that work by our author, were performed by introducing his hand and wrist under a glass jar, inverted in the mercurial trough.

1st—The jar being filled with quicksilver, he collected, in the space of sixteen hours, one-half ounce measure of air, which had escaped in small bubbles, from every point of the surface of the immersed hand. Two-thirds of this gas were absorbed by lime-water; the remainder suffered no diminution by the mixture of nitrous gas. *2d*—The experiment being repeated under water, a small quantity of azotic gas only was obtained, the carbonic acid having been absorbed in the water. *3d*—The jar contained seven ounces of azotic gas. At the

end of two hours, rather more than one ounce of carbonic acid gas was produced. Similar experiments were made with hydrogenous, nitrous, and oxygenous gases; and in every case the quantity of carbonic acid obtained, was nearly the same. "Experiment shows, then," says Mr. Abernethy, "that carbonic acid gas is *perspired* from the vessels; for into whatever air the hand be immersed, the quantity of carbonic acid given out will be nearly the same."

The venous blood is commonly supposed to owe its dark colour to the presence of an excess of carbon, which is excreted in the lungs for the purpose of uniting with the oxygen of the atmosphere. The arterial blood must, therefore, be deprived of this superabundant combustible matter, in order to acquire its florid hue and peculiar influence. Professor Abilgaard, of Sweden, however, completely refuted this idea by some careful experiments, which appeared to prove the very opposite state of things in regard to the two kinds of blood. Instead of detailing this important fact under the head of respiration, our author brings it into his chapter on *living action.*

It has been commonly supposed to be an established point, that the oxygen inhaled, corresponds to the carbonic acid exhaled; or, in other words, that the whole of the oxygen which disappears, is contained in the carbonic acid generated in respiration. From several quotations in relation to this subject, our author concludes, and apparently with great justice, that the conversion of 1.2 cubic inches of oxygen into the same quantity of carbonic acid gas takes place in every inspiration. He goes on, however, to observe,

"But if we pursue this subject further, we shall find that this proportion between the oxygen lost, and the carbonic acid generated, is not maintained. Sir H. Davy found that hydrogen gas could be respired with impunity, and that, in its passage through the lungs, it experienced no change. Here, then, was an easy mode of inspiring the residual instead of the atmospherical air, and what was the result? The bulk of this residual air reduced to the temperature of 55°, he estimates at 32 cubic inches, being composed of nitrogen 23, oxygen 4.9, carbonic acid 4.1, which is 15.3 per

cent. of oxygen, and 12.8 per cent. of carbonic acid. Now, in proportion as the carbonic acid increased from 8 to 12.8 per cent., the oxygen should have diminished from 13 to 8.2; but the carbonic acid has increased 4.8 per cent., and the oxygen, instead of being diminished 4.8 per cent., is increased 2.3, so that there is a surplus of 5 per cent. of oxygen."*

Mr. Ellis proved by his experiments, also, that the volume of oxygen which is lost, exceeds that of the carbonic acid thrown out by expiration; and, indeed, this excess was so apparent to Lavoisier, as to lead him to conclude that it was employed to form a part of the vapour of exhalation, by uniting with the hydrogen of the blood. In addition to these circumstances, our author brings forward the fact, that carbonic acid is generated by seeds, without any supply of oxygen, to show that it must be given out from the system, in the form of an excretion. Although we are not disposed to detract any thing from the force of such arguments, we cannot help expressing our surprise that Dr. Shute should have overlooked the circumstance that carbonic acid is actually contained in the blood. Sir Everard Home concluded from his experiments, that twelve cubic inches of this gas are contained in every eight ounces of blood; and Dr. Scudamore, although he could not detect so large a proportion, supposes that the fibrin is rendered soluble in the fluid parts of the blood, by the presence of the same agent. We regard this fact as conclusive upon the point under consideration, and shall not, therefore, attempt to analyze the numerous other circumstances discussed under this head by our author.

Upon the subject of the circulation, the author is not so bold as from the perusal of his introductory chapter we were led to anticipate. When we read of the process of circulation being a part of the grand digestive function, we fully expected to meet with something new and extraordinary in this chapter.

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* To understand the above, it should be recollected that Allen and Pepys have ascertained that 100 parts of expired air consist of nitrogen, 79; oxygen, 13; carbonic acid, 8.

The late experiments of Prevost and Dumas have proved that after the removal of the kidnies, the blood becomes impregnated with the peculiar qualities of urine. In obstructions and derangements of the structure of the liver, also, we frequently meet with the most undeniable symptoms of a bilious contamination of the fluids. These circumstances certainly render it probable, that the peculiar proximate principles of the various secretions, are elaborated in the blood as it circulates towards the different glands. Our author, however, does not take advantage of these circumstances to bolster up his favourite doctrine, but proceeds as follows:

"The blood is a fluid *sui generis*, that is, different from every other with which we are acquainted, consisting of certain substances derived through the medium of the absorbent system from every part of the animal frame, and through the medium of the respiratory organs from the surrounding atmosphere. All attempts to ascertain the nature and constitution of this fluid have hitherto proved fruitless. We are absolutely unacquainted with the nature and peculiar properties of those substances from whence it is immediately formed: we know, for instance, that it consists of a certain fluid derived from the intestinal canal, through the medium of the lacteal absorbents, of another fluid derived from every part of the body, through the medium of the lymphatic system, and of a certain substance derived through the medium of the respiratory organs from the surrounding atmosphere. We know that the first, denominated chyle, and that the last, denominated oxygen, are destined to be conveyed to the several parts of the system, for the purpose of being incorporated with it; and we have the strongest reasons for believing that the other substance, denominated lymph, having already accomplished its destined function in the system, is intended to be conveyed to different organs of the body, for the purpose of being eliminated from it; but we are absolutely unacquainted with the nature of chyle, oxygen, or lymph. This observation, as applied to oxygen may at first sight appear untenable; but we have every reason to suppose that oxygen does not unite with the blood so as to form a component part of that heterogeneous fluid, until it has previously united with some other substance, by which its characteristic properties are lost."

From this extract it will be seen that our author is very careful in discriminating between the recrementitious and excrementitious parts of the blood. Indeed, he goes so far, in one place, as to assert that the oxygen which is given out

from the lungs, in union with the carbon of the carbonic acid, is derived from the solid parts of the frame, in which it must have existed a sufficient length of time to have its active properties exhausted by constant use. We must confess, however, that this idea appears to us to be an over scrupulous piece of refinement, as far as regards the accuracy of division between the two kinds of particles. The old materials may, for aught we know, acquire new properties from being commingled with the circulating mass, so as to become again fitted for the purposes of nutrition. But even on the supposition that all those particles which have once been employed in the animal organization, are totally unfit for a renewal of their combination with the same parts, it does not involve the necessity of their being disqualified for a union with parts possessing a differently organized structure. To attribute to the capillaries a faculty of rejecting all the old particles of oxygen, and other elementary ingredients of the system, in favour of those recently introduced into the circulation, would require a greater degree of faith in the doctrines of Stahl, than we are willing to entertain. Our author, however, perseveres so diligently in hammering out this one of his most favourite opinions, that he occupies no inconsiderable proportion of several chapters with its details.

Mr. Hunter's doctrine of the vitality of the blood, is opposed by our author in round terms. "How can we consider," says he, "that to be a living action, which never takes place in a living system, except as the consequence of disease?" In thus alluding to the coagulation of the blood, which Mr. Hunter supposed to proceed from its vitality, he seems to think that he has hit the very nail on the head. He goes on, a little further, to sneer at the inconsistency of attributing life to the blood, for the purpose of producing that very same coagulation which the life of the vessels alone can prevent. This kind of reasoning, however, falls very far short of that which our readers will no doubt recollect to have been impressed with in our analysis of Dr. Scudamore's work upon the blood.

In the chapter on secretion, a very succinct and interesting view is given of the mechanical, chemical, and electrical theories of the causes by which that mysterious process is carried on in the animal economy. Of the chemical we would remark, however, that it should be comprehended under the electrical theory, inasmuch, as all changes in composition are now supposed by the chemists to be effected by the galvanic influence. In relation to the electrical theory, we extract the following passages, both because they are in themselves interesting, and because they will serve to show the state of medical opinion in Europe upon this subject:

"It must be admitted that the discoveries of Galvani, Volta, and others, have not produced that sudden revolution in the science of medicine, which was anticipated by these celebrated philosophers, when the phenomena of galvanic electricity were first observed; but it appears to me, that the progress towards that revolution is clearly discernible, is every day more apparent, and I am sanguine enough to expect that it will ultimately be complete. A considerable period of time has now elapsed, since the early experimentalists, yielding to that enthusiastic spirit of discovery which too often dazzles but to deceive, boldly asserted that the galvanic influence was life; but man, though often led away by his feelings, is a rational animal, and he soon discovered that the galvanic fluid was *not* life. We all know that if a pendulum be impelled to a certain distance on one side of its centre of motion, it will, by its natural reaction, return to the same distance on the other; so has it been with the galvanic philosophers; the galvanic fluid was not *life*, and therefore it was *nothing;* galvanism did not *regulate* the actions of the system, and therefore it had *nothing* to do with them. But the vibrations of the pendulum are contracting upon their centre, and we are gradually withdrawing ourselves from the extremes of enthusiasm on the one hand, and of despair on the other. We now admit that galvanism is neither life, nor the comptroller of living action, but at the same time feel convinced, that it has an intimate connexion with both."

"Mr. Abernethy says,—'I consider it already *demonstrated* that they are electrical actions which cause the various combinations and decompositions so constantly occurring in all parts of the body.'"

"Sir Everard Home says,—'By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid and the salts of the blood, that with one degree of power albumen is separated in a solid form, with a less degree it is separated in a fluid form.'"

"Such decomposition of the blood by electricity, may be as near an approach to secretion, as could be expected to be produced by the artificial means at present in our power. The discovery of an electrical power which can separate albumen from the blood in a fluid state, may explain the mode in which different animal solids and fluids may be produced: since, according to Mr. Hatchett's experiments, albumen is the principal material of which animal bodies are composed."

Dr. Young has attempted to inquire a little more minutely into the nature of secretion, with what success he leaves his readers to determine for themselves.

"We may imagine, that at the subdivision of a minute artery, a nervous filament pierces it on one side, and affords a pole positively electrical, and another opposite filament, a negative pole; the particles of oxygen and nitrogen contained in the blood, being most attracted by the positive point, tend towards the branch which is nearest to it, while those of the hydrogen and carbon take the opposite channel; and that both these portions may again be subdivided, if it be required, and the fluid thus *analysed* may be *re-combined* with new forms, by the re-union of a certain number of each of the kinds of minute ramifications. In some cases, the apparatus may be somewhat more simple than this; in others, perhaps, much more complicated; but we cannot expect to trace the processes of nature through every step; we only inquire into the general direction of the path she pursues, as much in order to avoid being led away by false opinions, as for the sake of any direct advantage that can be gained from our partial views of the true state of the operation."

Our author attributes some degree of weight to the suggestion of Dr. Ontyd,—that electricity unites with the living power, and becomes divested of some of its natural properties, by means of which union it becomes *animal electricity*. He concludes that all the theories of secretion agree in the point, that the principles of the blood must be "disunited, and recombined in a new manner, and in different proportions." This circumstance he takes good care to dispose of in such a way as to bear upon his favourite digestive notion. He thinks that secretion is the same thing as digestion, and is performed by the very same agent; the only difference consisting in the circumstance that food is concerned in one case, and blood in the other.

In the chapters on excretion and nutrition, we can discover nothing peculiar, except that our author labours hard to prove that the former process is wholly devoted to the removal of the effete particles from the system; while the latter is equally confined to the appropriation of those newly introduced to the several parts of the whole animal structure. Upon the uncertainty of our knowledge upon this subject, we have already remarked, and we shall, therefore, avoid further comment upon it on the present occasion.

In the chapter on electricity, some interesting facts are detailed respecting the natural accumulation of that fluid in living bodies. In cold climates, there is always a disposition to this phenomenon under certain circumstances; but such cases as our author has extracted from various publications, are not witnessed by many observers. The former grand duke of Russia could give sparks from his hands and face; and Count Orloff could almost light up a room by drawing a comb through his hair. On the top of Mont Blanc, Messrs. Pictet, Jalabert, Saussure, and servants, could hear the electric fluid pass off from their buttons with a buzzing noise; while their fingers at the same time smarted with the streams which were passing off from them. Messrs. Tupper, Lanfier, and guide, on descending Mount Ætna, found that musical sounds were produced by the rapid passage of electricity from their hands to their heads, &c. which could be heard to the distance of forty feet. This last mentioned fact is particularly interesting, inasmuch as it proves that there may be different quantities of electricity existing in different parts of the body at the same period of time. The phenomena presented by the torpedo and gymnotus electricus, however, prove the same thing much more conclusively. The experiments of Humboldt, and Gay Lussac, have proved that animal electricity is very different, in some particulars, from common electricity. They ascertained that the torpedo might be touched with impunity, by means of a key, or any other metallic body; and that an electrometer of the most sensible

kind was not at all affected by the presence of the fish. Mr. Flagg also found "that the animal might be touched with a metallic rod enveloped in a silk handkerchief, with impunity," and many others have observed, that the shock could not be transmitted through two or more persons, "unless they wetted their hands." Our author explains these facts by supposing that the electric fluid "undergoes *certain changes*, and is modified by the actions of the living system, by which it acquires properties peculiar to itself, and becomes in reality, not the electricity of the atmosphere, but *animal* electricity."

It is a singular fact that, after the torpedo has lost its electrical powers under water, they can be restored again by exposing it to the air. Mr. Flagg ascertained that this fish "raises its head every five minutes above the water, as if to respire." These circumstances appear to prove that animal electricity must be derived from the air in respiration.

After being stunned by a blow on the head, the torpedo loses its power of communicating a shock. Our author draws from this fact the inference that the shock does not arise from the general accumulation of electricity in the system, but from the influence of the will. He presumes that it is capable of being concentrated by the will of the animal, and that this power is intimately connected with its nervous system. He quotes the dissections of John Hunter, which detected an "immense size of the nerves that go to the electrical organs." Sir Everard Home has proved that "in these fish, the nerves connected with the electrical organ exceed those that go to all other parts of the fish, in the proportion of *twenty to one.*"

Dr. Shute thinks that the free electricity with which a man becomes charged by standing near a highly electrified body, has very little influence upon the system. He says that this fluid "thus *undigested*," affects animal matter no more than a living seed which passes unchanged through the bowels. The body must, therefore, be constantly charged with two

kinds, or modifications, of electricity,—the one free, the other combined with "some other substance," in such a proportion as to alter its ordinary properties. This leads us to the consideration of the very sum and essence of all our author's reasoning,—the nature of the *nervous influence.* It is contained in the middle of his book, to which the fore-part is merely introductory, and from which the after-part is drawn out like the application of an interminable sermon.

The common opinion respecting the nature of the nervous influence appears to be, that it is either pure electricity, or some modification of that fluid, by means of which the various organs of the system are excited into action. Irritability, or the power of being excited, is supposed, on the contrary, to depend on the presence of the oxygen, which distinguishes arterial blood. The source of both these agents is attributed to respiration, by which they are imagined to be introduced into the blood for the purpose of being distributed to all parts of the body. According to our author, however, the nervous and the arterial influences are both constituted by the same agent; and this he believes to be a combination of certain proportions of electricity and oxygen.

"Living action may be dependent upon oxygen *and* electricity, but not upon either exclusively. Millions of animated beings have been sacrificed upon the altar of experimental inquiry, and what is the result? We cannot at this moment determine, whether the action of the heart be dependent upon the arterial or the nervous influence, or whether it may not be dependent upon both. Is not this, at least, a presumptive proof that the arterial and nervous influence may be the same things? In support of the opinion that the nervous influence may be a peculiar combination of oxygen and electricity, I may adduce the example of galvanism."

To support this analogy between galvanism and the nervous influence, he resorts to the well known fact, that after an organ has lost its power of acting, in consequence of a division of its nerve, if the galvanic influence be applied to the inferior portion of the divided nerve, the organ *recovers* its power of being excited into action, by an appropriate stimu-

lus. After citing Mr. Philip's experiments on the stomach, he proceeds to say,

"If these experiments do not point out the identity of, they at least establish the striking analogy between the galvanic and the nervous influence. If, therefore, we have reason to suppose that the galvanic influence may be a peculiar combination of oxygen with electricity, we have the strongest possible analogical proof that the nervous influence is also a combination of these two substances. The nervous and the galvanic influence resemble each other in the effects they produce, and, therefore, we have the strongest possible reason for supposing that they resemble each other in the nature of the principles which enter into their composition."

To prove that galvanism is a compound of electricity and oxygen, he draws all sorts of logical inferences from Mr. Cruickshank's theory respecting *oxygenated* and *de-oxygenated electricity*. The only substantial argument, however, which he has advanced in relation to this point, is, that the presence of oxygen is always necessary to the development of galvanism. So invariably, indeed, is this the case, that many chemists have been driven to the conclusion, that the electrical phenomena which accompany various chemical decompositions, are produced by some change in the state of the oxygen, that exists in all substances capable of undergoing such decomposition. Now, as the presence of oxygen is equally necessary to the production of arterial blood, our author thinks himself entitled to draw the conclusion, that galvanism and the arterial influence are the same things. In confirmation of this opinion, he has brought forward a very interesting fact, which is the only one, as far as we have been able to discover in the whole book, that he has taken the trouble to put to the test of experiment. It is that galvanism converts venous into arterial blood.

"The analogy is still further pointed out by experiment. The peculiar property of the arterial influence, is to change the dark hue of venous, into the scarlet colour of arterial blood. The galvanic influence will also produce this change in the colour of venous blood. If the wires attached to each end of the galvanic trough, be brought into contact, or into a certain state of approximation, under the surface of venous blood, bubbles of a

vivid scarlet hue immediately rise to the surface. As often as the experiment is repeated, the same phenomenon may be observed. If a small portion of the dark coagulum of venous blood be allowed to attach itself to each wire, that of the positive wire will assume a brighter appearance, and that of the negative wire will be little, if at all, altered in its colour; but as soon as the two wires are brought into contact, the coagulum attached to each will assume a bright scarlet colour."

Mr. Wilkinson and Mr. Brande have made the same observations. The experiments of the former gentleman are detailed in his essay on galvanism, those of Mr. Brande were published in the Philosophical Transactions for 1809.

The next point which our author attempts to prove, is, that the arterial and nervous influences are the same things. The nervous influence he denominates "the secondary source of power;" and he supposes it to be abstracted from the arteries, and accumulated in the nerves, not for the purpose of exciting, but for that of supporting the vital properties. He arranges the points of analogy between these two influences under the following heads:

"Oxygen is necessary to the excitement of both. They both influence living action in the same manner.

"There is reason to suppose that they change the colour of the blood in the same manner.

"There is reason to suppose that they both contain electricity.

"There are no facts which point out any dissimilarity between the two, considered either as to the nature of their constituent principles, or as to their agency in the animal system.

"And lastly, the phenomena of living action seem to indicate the necessity of this secondary source of power. The effect being the same, we have a right to presume that the course is also the same."

Upon the second of these analogies, that they change the colour of the blood in the same manner, he throws out some curious hints. He thinks that the sudden change from dark to red blood, which occasionally takes place in venesection, results from an immediate communication of the galvanic influence of the nerves to the venous blood, just before it escapes from the orifice. He supposes, also, that in inflammation all the blood, which returns from the inflamed part is

reddened or arterialized in the same way. He demonstrates the necessity of a secondary source of power in the nerves, by a reference to the state of the heart while it is recovering from syncope, or suspended animation.

"Supposing the action of the heart to be suspended, or even diminished, how could its power be restored under any circumstances? The power of the heart, according to this theory, (the theory of Bichat) is wholly dependent upon itself; that is, upon the supply of the arterial influence through the medium of the coronary arteries, which supply pre-supposes an action of the heart itself. Thus the heart must act in order to acquire the power of doing so. Or supposing its action to be diminished, a weak action of the heart implies a diminished supply of the arterial influence. There can be no increased supply of the arterial influence without an increased action of the heart; so that the heart must act more powerfully, in order to acquire the power of doing so. But this increased action of the heart is a debilitating process; is itself a source of exhaustion; so that the heart must exhaust itself still further, as the only means of acquiring power. Is it not manifest that there must be some *other* source of power? It has been supposed that under these circumstances the nervous influence may act as a stimulus to the heart; but increased action, the effect of a stimulus, is itself a source of exhaustion: so that the heart must still exhaust itself as the only means of acquiring power. What is the simple explanation of the fact? The nervous influence is a secondary source of power, is a secondary source of the arterial influence. This unites with the heart independently of any increased action of the heart itself, and thus imparts power without the necessity of any debilitating action of that organ. In cases of suspended animation, we have resource to the artificial supply of oxygen through the medium of the respiratory organs: in fainting we apply a stimulus to the nervous system, and we succeed in restoring the action of the heart by these apparently different remedies. The heart itself, in both these cases, is in the same state. Its irritability must be restored. How do these different remedies produce the same effect? If the nervous and arterial influences be the same things, the question is easily answered."

It is hardly necessary to proceed further with our author's views. He has introduced galvanism fairly into the system; and with such an all-pervading agent an ingenious theorist can do any thing within the bounds of imagination, or of possibility.

M.

ART. V.—*A Compendious System of Midwifery, chiefly designed to facilitate the inquiries of those who may be pursuing this branch of Study, &c.* By W. P. DEWEES, M. D., Lecturer of Midwifery, &c. Philadelphia. H. C. Carey & I. Lea, 1824. Octavo, pp. 602.

AMONG the multitude of new medical works that are continually issuing from the press, we find but few, comparatively speaking, which deserve to be regarded as real accessions to our literature. The mass of clumsy compilation, of senseless theorising, and imperfect experience, which is constantly obtruded on the attention of the profession, is such as to alarm even omnivorous dullness itself. Now and then, however, we are presented with works of a different and more useful character,—with practical books, which give the result of a long course of enlightened observation and experience,—or with well directed and rational disquisitions on subjects of a theoretical nature. It is among this latter, and more useful class of publications, that we would rank the work whose title is at the head of this article. It is by no means a mere compilation of the opinions and experience of other writers. Its merits are of a higher order, being, in a great measure, drawn from the author's own extensive experience and reflections upon the subjects which it treats. Much, however, as we admire the many sound practical lessons contained in this book, we cannot help remarking, that in point of language, it falls short of what we had reason to expect from the author. It is undoubtedly true, that the mere literary merit of a practical book, is but of secondary consideration,—and that such a book is valuable in proportion to the value of its materials. We are not, therefore, disposed to dwell with a censorious spirit on the style of this work, which, though not to our taste, is sufficiently perspicuous and lively, to enable any one to read it without fatigue or irksomeness from this source. Whatever defect we may suppose it to have in this respect, we feel assured, that it is calculated by its intrinsic worth to reflect much credit on its author; and we take

pleasure in thus offering our testimony in behalf of its excellence. We proceed to a short notice of some of the more interesting portions of the work. The first six chapters are devoted to the consideration,—1*st*, *Of the Pelvis;*—2*d*, *Of the Child's Head;*—3*d*, *Of the Genital Organs;*—4*th*, *Of Menses;*—5*th*, *Of Conception*, and 6*th*, *Of the Action of the Uterus.*—In the 7*th* chapter, the author treats of the displacements of the uterus. *Retroversion* of this organ, according to Denman and Merriman, is not an accident of serious moment. Hunter, Baudaloque, Burns, and other writers, on the contrary, look upon it in a very different light. Dr. Dewees, also, is directly opposed to the two former gentlemen on this point, and he has given, we think, a satisfactory refutation of their opinions.

The symptoms of this complaint are more or less intense, according to the size which the uterus may have acquired, or according to the suddenness or slowness with which the retroversion is effected. "When suddenly induced, the symptoms are usually violent and alarming,—such as an immediate interruption to the flow of urine, or the passage of the fæces; alternate pains, accompanied by great forcing or bearing down; a disposition to fainting, &c. When considerable time is spent in completing this displacement, the evils arising from it are less urgent and severe. But in both cases, if the uterus be not restored, the symptoms will increase in intensity; instead of merely a difficulty and frequent inclination to make water, there will be a total suppression of it, accompanied by a painfully intense desire to do so, for the fœtus will go on to increase in size, and the uterus to develop itself, giving additional pressure to the parts with which it is in contact."

Dr. Denman thinks, that retroversion of the uterus may, in every instance, be referred to a distended bladder for its cause, and "that suppression of urine is absolute only before, or during the act of retroversion," and must therefore be regarded as the cause, and not the consequence, of the com-

plaint. Dr. Dewees objects to this opinion, because: "1*st*, we are certain that it has been suddenly produced by violence, and without the intervention of a suppression of urine. 2*d*, because Baudaloque demonstrated, before his class, a slow retroversion of the uterus, which lasted three or four weeks before it was complete; in this case there is no mention of any difficulty in making water."

According to Denman, every suppression of urine, occuring about the third or fourth month of pregnancy, and "continuing a *certain* length of time," with a certain degree of distention of the bladder, should be regarded as a case of retroverted uterus. Dr. Denman, forming his diagnosis of this complaint upon such grounds, we need not wonder, observes Dr. Dewees, that he considers this as a trifling disease. For "should every suppression of urine in a pregnant woman really constitute retroversion, as is declared by Dr. Denman, we can readily account for his indifference to its consequences, and his trusting its cure to nature, or the occasional drawing off the water by the catheter."

When retroversion of the uterus has taken place, we may generally succeed in restoring the fundus to its proper position, by the occasional employment of the catheter, and of mild laxatives or injections. If we fail in removing the complaint by these means, "we are then maturely to consider the propriety of mechanically replacing the fundus." There are three circumstances, says Dr. Dewees, which must be taken into view, in deciding upon this point. 1*st*, The period of gestation;—2*d*, The degree of development of the uterus;—and 3*d*, The nature or severity of the symptoms. The nearer the accident occurs to the fourth month of gestation, the greater will be the necessity for a prompt restoration of the retroverted fundus. As some uteri are much more developed at three months, than others are at four, the development of this organ must also be taken into consideration when deciding upon this point; for where the womb is more expanded than it commonly is in other cases at the same pe-

riod, there will of course be a greater necessity for prompt interference. Where the symptoms are exceedingly severe, and the water cannot be drawn off with the catheter, in consequence of the impossibility of passing it into this viscus, we must take care "not to temporize too long, lest the bladder become inflamed, gangrenous, or burst," all of which terminations have been witnessed.

"From this we conclude," says Dr. D., "that the uterus should in every instance be restored when practicable, at, or very little after the fourth month; if left longer than this, the risk of not succeeding is every day increased; and we are firmly of opinion, that nothing can justify a neglect to do so at this time, more especially when it proceeds from the vain hope, that nature will relieve herself at the full period of gestation."

That the bladder may be much and long distended, from retention of urine, without producing retroversion of the uterus, Dr. D. is quite certain from his own experience; and he is equally convinced, "that in retroversion, the mere removal of the urine will but rarely, nay, not once, perhaps, in ten times, *where the complaint is of long standing, or the pregnancy advanced beyond the third month,* be sufficient to ensure the spontaneous restoration of the fundus." It must nevertheless be borne in mind, that the urine is always to be drawn off, where practicable, as often "as the exigencies of the case may require." This is indispensable either for the spontaneous or artificial reposition of the uterus.

Dr. D. thinks that the risk of abortion is but trifling; and that the apprehension of this consequence should not deter us from attempting to restore the uterus to its proper position.

The directions given by our author, for the best mode of performing this operation, are precise and judicious. The forces which resist the restoration of the displaced fundus, are:—"1*st*, A distended bladder.—2*d*, An impacted rectum. 3*d*, The counteracting efforts of the woman herself.—4*th*, The too great bulk of the uterus." The first thing to be done, therefore, is to remove these obstacles as much as possible. To obviate the resistance offered by the involuntary efforts of the

woman to bear down, Dr. Dewees recommends bleeding "to, or near to, fainting." When bleeding is determined on, "we should be prepared before hand to take advantage of the deliquium, as its effects are but transitory." Every thing should therefore be prepared in such a manner as to enable the accoucheur to proceed to the operation without delay or inconvenience during the period of syncope.

When blood has been drawn until faintness is induced, and the woman is placed in a proper position,

"The hand, after being well lubricated, should be passed into the vagina, to a state of supination, the fingers retracted in such a manner as to form a straight line at their extremities; they must then be gently pressed against the base, as it were, of the tumour that is found within the vagina, so as to move it backwards and upwards along the hollow of the sacrum, until the mass shall reach above the projection of this bone; when thus far, the hand may be withdrawn, and a pessary be introduced of a proper size. The woman must remain quietly in bed for three or four days; the urine for this period should be drawn off as often as may be required, and the fæces evacuated by injections."

The author's observations "*on the obliquities of the uterus*," are interesting, and deserve to be attentively read. When the fundus of the uterus continues to advance, "in the direction it received when passing through the superior strait," without being sufficiently resisted by the pressure of the abdominal parieties, it acquires what is termed the "anterior obliquity." The abdomen becomes pendulous, which is sometimes carried to a surprising extent, "especially in little women who are much upon their feet, and in those who have a deformity of pelvis." The inconveniences of this condition, in the latter period of gestation, are often very great; such as severe pain in the back, loins, and hips, with a distressing sensation of bearing down.

This anterior projection of the uterus is sometimes a source of considerable embarrassment to the midwife, as well as of distress to the patient. The os uteri, in cases of this kind, is often thrown so far out of the proper axis of the pelvis,—being turned back towards the projection of the sacrum, that

the head will descend into the pelvis, "covered by the stretched anterior portion of the uterus." It is therefore necessary to attend early to the reduction of the os uteri to its proper position. To do this, in cases where the mouth of the uterus cannot be reached in the ordinary way, the hand must be introduced, "so that its palm may be next to the distended uterus;" a finger is next to be hooked into the os uteri, which is then to be drawn "towards the centre of the interior strait. When it has followed so far, the hand may be gently withdrawn, but not the finger from the os uteri, and the uterus detained there until the proper direction of the forces, and the axis of the uterus, are made to correspond."

The distressing consequences which may result from the want of a proper knowledge of this variety of uterine obliquity, is strongly exemplified by the following relation of our author:

"Within our own knowledge, this case has been mistaken for an occlusion of the os uteri, and where, upon consultation, it was determined that the uterus should be cut, to make an artificial opening for the fœtus to pass through; they thought themselves justified in this opinion, first, by no os uteri being discoverable by the most diligent search for it; and secondly, by the head being about to engage under the arch of the pubes, covered by the womb. Accordingly, the labia were separated, and the uterine tumour brought into view; an incision was now made by a scalpel through the whole length of the exposed tumour down to the head of the child,—the liquor amnii was evacuated, and, in due course of time, the artificial opening was dilated sufficiently to give passage to the child; the woman recovered, and to the disgrace of the accoucheurs who attended her, was delivered, per vias naturales, of several children afterwards; a damning proof that the operation was most wantonly performed."

Dr. D. observes, that it is sometimes sufficient for the restoration of the fundus, even in cases of extreme obliquity, simply to place the woman upon the back; the pendulous abdomen being at the same time raised and supported by means of a towel or napkin properly adjusted.

In the eighth chapter, the author treats of the signs "which usually attend pregnancy." He warmly, and, we think, suc-

cessfully, combats Dr. Denman's opinion, that the continuance of the menstrual discharges insures an exemption from pregnancy. It appears to us extraordinary indeed, that any one who has had Dr. Denman's opportunities of observation, should advocate this opinion. Dr. Dewees avers, that he knows many women "who habitually menstruate during pregnancy until a certain period. Several of these menstruated until the second or third month, others longer, and two until the seventh month." Within this year, we have ourselves seen two instances in which the menses appeared regularly, and in their genuine character; in one of them twice, and in the other four times, after the period of conception. The latter female informed me that the same circumstance took place in her two former pregnancies. Other writers mention similar cases; Dr. Dewees refers to Heberden, Hosack, and Francis, for evidence on this point. When facts of this kind can be adduced, it is useless to reason either for or against the possibility of such an occurrence. Dr. Dewees mentions a case that came under his notice, in which menstruation went on regularly during pregnancy, but ceased when the woman was not in this state. He observes, that so far as he has been able to ascertain, Deventer is the only writer who has furnished a similar example. Froriep, a late German writer on midwifery, refers to a case of this kind, recorded in Stein's Annals on Midwifery, Vol. I. p. 166. In this instance, menstruation was perfect and regular throughout the whole period of gestation, but was always wholly absent when the woman was in an unimpregnated state.*

The heart-burn which sometimes attends the early period of pregnancy, may be often effectually relieved, says Dr. Dewees, by eating a few blanched almonds, or roasted groundnuts. "These should be taken from time to time, as the acidity may prevail. It is of much importance, in cases of

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* Handbuch der Geburtshuilfe, von Lud. F. V. Froriep. Weimar, 1822, page 178. See, also, Stein's Annals of Midwifery, Vol. I. page 166.

this kind, to guard against constipation of the bowels.—Dr. D. has occasionally found the use of ship-bread, or of biscuit made of unbolted flour, to answer very well in such cases. Where this is insufficient, he advises the use of the following pills, which, when "properly persevered in," are often of great advantage:

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	G. Aloes, Soc.	-	-	-	-	ʒss
	Pulv. Rhæi	-	-	-	-	ʒj
	Ol. Caryoph.	-	-	-	-	gr. iv
	Sapo. Venet.	-	-	-	-	gr. viii
	Syr. Rhæi	-	-	-	-	g. s. ut fiant pil. xxx.

One of these may be taken every night.

For the leucorrhœal discharge, which is often found to accompany pregnancy, Dr. Dewees directs the parts to be washed three or four times a-day, with *luke-warm* water, and a weak solution of sugar of lead to be thrown into the vagina with a syringe. Before the injection is used, however, "the parts should be washed with a weak solution of fine soap in warm water," by throwing a few syringes full into the vagina. "We are convinced," says Dr. D., "that this last plan will afford relief when the non-observance of it might not be followed by the smallest benefit." Dr. D. prefers *luke-warm* water to cold; and in this he differs, as he himself observes, "from almost every other practitioner." From long experience, however, he is fully convinced of the superiority of the former.

Our author's remarks on the treatment of fluor albus, in females, not pregnant, are well worth attending to. He divides the idiopathic forms of this complaint into three stages, "each of which requires a little difference of management."

"In the first, or most simple form, the matter discharged is glairy and transparent, or resembling a thin starch; this very often accumulates, from its tenacity, in considerable quantity within the vagina, and is then suddenly discharged, either by its own weight, or by some sudden effort of the woman, especially upon stooping, or lifting a weight,—this never becomes acrid, unless there is the most reprehensible neglect of cleanliness, nor so far as we have observed, is the system implicated, though it may take place

in constitutionally plethoric women. We always commence with a direction that the parts are to be regularly washed with warm water, three or four times a day,—if the patient be plethoric, we cause her to be well purged; confine her to a milk and vegetable diet, and sometimes order her to be bled; when the pulse is sufficiently reduced by these means, or if the pulse be in a proper condition without these means, we then commence with the tincture of cantharides; of this we direct thirty drops every morning, noon, and evening, in a little sugar and water, increasing the dose every third day, five drops at a time, until strangury is produced. We always direct our patient to desist from the use of the tincture, so soon as she feels the approach of strangury, and not to resume it again until all uneasiness disappears. Should the complaint withstand the first strangury, we are not discouraged, but recommence the tincture, of the original dose of thirty drops, and increase it as before, until a difficulty in making water is again experienced,—it rarely, however, withstands the second irritation of the bladder."

As soon as the discharge becomes thinner and more abundant, he employs astringent injections. These, however, should never be used until this change in the discharge has taken place.* He prefers, in cases of this kind, the acetate of zinc, in the proportion of five or six grains to an ounce of water. The second stage, or variety of this complaint, is characterized by a discharge of a "white or yellowish purulent appearance." From want of proper attention to cleanliness, this discharge may become offensive and excoriating. This form of the complaint is "always accompanied with pain in the back, hips, and in the region of the pubis; the woman's complexion is generally sallow, and when the discharge is excessive, she becomes subject to a train of distressing nervous symptoms." The pulse is almost always found to be hard, wiry, and irritated. Brisk catharsis, a vegetable diet, and bleeding, should never be neglected in the commencement of the treatment. After the pulse has been brought to a proper state by these means, the author directs the employment of the tincture of cantharides, as stated above, with saturnine injections.

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* The tincture of cantharides employed by Dr. Dewees, is "just fifty per cent. stronger than the ordinary tincture of the shops."

In the third stage, all the symptoms of the second are much aggravated. The discharge has a green colour, and is often tinged with blood.

The same plan of treatment is required in this form of the complaint, as in the preceding one, with this difference only, that the antiphlogistic remedies in the commencement must be more actively employed, and the dose of the tincture of cantharides more slowly and cautiously increased.

The *pruritus*, or itching in the pudendum, to which females are subject, especially during pregnancy, is sometimes exceedingly distressing.

"The precise nature of this affection," says Dr. D. "so far as we know, has never been pointed out; and accident furnished us with an opportunity of detecting the real condition of the parts where this complaint was in full force. A lady, whose husband was more notorious for his gallantries, than his domestic virtues, was attacked in the incipient stage of pregnancy with an intolerable itching in the pudendum, and even within the os externum along the vagina. Suspecting she was infected by a venereal affection, we were sent for, and she giving such an account of her feelings as to make us think it might truly be the case, we proposed an examination of the parts, which was finally acceded to. Upon separating the labia, the whole face of the vulva, the os externum, and as far in the vagina as could be viewed, *was covered with an incrustation of apthæ.* We assured the patient her complaint was not as she had supposed, but one which we hoped we could quickly remove. We accordingly ordered a strong solution of borax in water, and requested her to wash herself four or five times a-day with it, as well as to throw some of it up the vagina at the same time; she did so, and was perfectly well in twenty-four hours."

Dr. Dewees considers borax as nearly a certain remedy in this complaint. In obstinate cases, bleeding and purging are important auxiliaries.

The chapter on *Deranged Menstruation*, contains some novel and very judicious observations, and is full of sound practical advice. Some of the author's remarks "on the tardy appearance of the menses," appear to us particularly important.

"There seem," says Dr. D., "to be four conditions of the female system, in which the menses are tardy in their appearance:—1*st*. Where there is little or no development of the

genital organs;—2*d*, where this development is interrupted by a chronic affection of some part;—3*d*, where the most perfect development has taken place, but they do not make their appearance."

The first condition of the system is known by the absence of those signs which characterise puberty. "The breasts are not swelled, but remain dormant, as it were; nor is hair always protruded on the pubis."

"In a girl thus circumstanced," says Dr. D. "who, otherwise than the mere absence of the menses, is in good health, it would be more than idle, it would be cruel and dishonest, merely because she had attained her fourteenth and fifteenth year, to subject her to medical rule, or goad her system by stimulating emmenagogues. In such a case, if the mother or friends are rational, and to be trusted, we may honestly give our opinion of the entire insufficiency of medicine to produce the desired end. We should explain, so far as we can, the nature of the functions of menstruation, and of the pre-requisites to this discharge, and attempt to produce on their minds the important conviction, that time, under proper circumstances, is all that is required to effect the anxiously hoped for change. Where the friends, however, are importunate, and will not yield to this salutary advice, we should use a temporising plan, and, by the administration of some entirely inert medicine, gain time, and save the poor child from permanent ill health, or an untimely grave."

We have not room to give a particular account of what our author says on "the suppression of the menses;"—"the immoderate flow of the menses,"—"dysmenorrhæa,"—and "the decline of the menses." We cannot, however, forego quoting his remarks on the employment of *aloes* in that species of uterine hæmorrhage, which sometimes occurs about the period of the cessasion of the menses.

"Against the use of aloes," he observes, "there is much clamour; but we have some reason to believe, not with sufficient justice. We do not wish, by any means, to decide this point at this time, as our experience in its use is as yet rather limited. We have thought proper to direct attention to it, that we may be aided by the experience of others, in determining the powers of this medicine. But we will relate what we know upon the subject, and leave it to the further employment of this drug to either confirm or destroy our present favourable impression of it. A lady, aged forty-two

years, for whom we had prescribed almost all the known remedies for the hæmorrhage under consideration, with very little benefit, was told by some old women, that the *hiera picra* was a certain cure in her complaint; she mentioned this to us, and we very candidly stated our own and the general prejudices against the principal ingredient in this compound; but, at the same time observed, that as the old woman who had recommended it, cited the cases of two or three ladies who were known to her, it would be easy to make the inquiry, and if it was as she stated, it would be well, as every thing else had failed, to give it a trial. The medicine was warmly recommended by these ladies, and she proceeded to make use of the old woman's prescription, which was half an ounce of hiera picra to a pint of gin; of this a wine-glass full was directed at bed time;—it was taken, and the lady was completely intoxicated all night, and very sick in the morning. To avoid this sickening effect of the medicine, we prescribed it for her in the following manner:

"℞	Hiera Picra - - -	ʒjj
	Ol. Caryoph. - -	gr. x
	Sapo. Venet. - -	gr. xjj
	Syr. Rhæi - - -	g. s. ut fiant pil. xxxx.

"One of these was directed every morning, noon, and evening, unless they should prove too purgative. She soon perceived a diminution of the discharge, and by the time she had finished the pills, it was so much reduced in quantity, as to give no further uneasiness. Two cases of a similar kind, but of less severity, were entirely relieved after the use of the same formula."

As a general remedy in cases of this kind, Dr. Dewees has found the extract of cicuta highly serviceable. It should be commenced with in small doses, and gradually increased until marks of "its influence, such as vertigo, head-ache, or sickness of the stomach, begin to show themselves." When these symptoms are observed, the medicine is continued without increasing the dose, until they go off, when it must be again increased," and so on, until the complaint has so far yielded, as to render its farther exhibition unnecessary, or until we are convinced that it will not succeed in arresting it. Dr. D. reprobates the employment of tonics in this complaint, as highly injurious in the majority of cases. "They are only admissible," he observes, "where there is nothing but debility to contend with."

Chapter XII. *Of Labour.*—The rigors which some females experience in the very commencement, or during the progress of labour, seem, says Dr. Dewees, to be connected "in some way or other, with the dilatation of the os uteri." When these shiverings, (which are never attended with the sensation of cold,) take place during the progress of labour, Dr. D. "has always felt assured that the mouth of the uterus was dilating, or dilated."

With regard to the manner in which the os uteri is dilated, during parturition, Dr. D. has offered an explanation, which is both ingenious and plausible. Want of room, however, obliges us to omit an account of the Dr's. peculiar views on this subject; we therefore refer the reader to the work itself, for a satisfactory explanation on this head. We will merely observe, that he does not consider this phenomenon as the consequence of mechanical force, exerted by the distended membranes, or any portion of the child's head, opening the mouth of the uterus by a wedge-like action.

In relation to the "*after pains,*" the author observes, "that by the old women, (and we may add, by many accoucheurs also) the after pains are considered useful, because they are almost always accompanied by the discharge of coagula, which they say must come away, and on this account oftentimes refuse to give any thing for their relief. But we have always regarded them as an evil of great magnitude; we always endeavour to arrest them as quickly as possible,—they interrupt sleep, which is at this time of special importance, and they also needlessly excruciate the body." In these sentiments we perfectly agree with Dr. Dewees. He generally prescribes camphor for the relief of these pains, according to the following form:

"℞	G. Camph. - - -	℥jj
	Spir. Vin. rect. - -	g. s. f. pulv. adde
	Pulv. G. Arab. - -	℥jjj
	Sacch. Alb. - - -	g. s.
	Aq. font. - - -	℥vi. M.

"A table-spoonful of this mixture is to be given every hour or two, until ease is obtained."

Dr. Dewees states that he has seen a few cases of a peculiar and very distressing character, which he has not found noticed in any of the books on midwifery. "It is a most severe and constant pain at the very extremity of the sacrum and coccyx; it begins the instant the child is born, and perseveres with most agonising severity, until its violence is overcome by the rapid and liberal use of camphor and opium. It is declared by the patient, to be infinitely more insupportable than any pains of labour, for it is never-ceasing." In one case Dr. D. was obliged to administer nearly *five hundred drops* of laudanum before relief was obtained.

This same lady was similarly affected in her second and third confinements. In the second, laudanum was given in large and repeated doses, "which *eventually* overcame the pain." In the third confinement, however, the sufferings of the patient were much more speedily relieved, by administering a table-spoonful of the following julap every fifteen minutes, by which the pain was entirely subdued in the course of an hour:

"℞	P. G. Camph. - - -	℥jj
	P. G. Arab. - - -	℥jjj
	Tinct. Opii acetat. - -	℥jjss
	Ol. junip. - - - -	gr. xx
	Sacch. Alb. - - -	g. s.
	Aq. fontanæ - - -	ℨvi. M.

Another case which fell under the Dr's. notice, was treated with this julap, and the result was equally speedy and favourable.

Under the head of "*Exhaustion*," in his chapter on "*the causes of preternatural labours*," the author makes some remarks which appear to us worthy of much attention. The "*exhaustion*" of which he here speaks, "is not the mere loss of strength in the muscular system, but a state of inertia of the uterus itself." This state of inertia of the womb

may arise from different causes, and require different modes of treatment. It may be the consequence of an over distention of the uterus, from an excess of the liquor amnii. In cases of this kind, the pains return at irregular intervals, and are "confined to the uterine globe, which is excessively distended, with very little of that bearing down sensation, which accompanies the healthy protrusive effort. The membranes are not felt to become very dense during pain, and the woman experiences a general feeling of anxiety and restlessness during the intervals of the pains. "In this case, the loss of power is only relative, and though it may have all the appearance of absolute weakness, and is frequently mistaken for it, yet it is not truly so. The remedy in this case is, to remove the cause by evacuating the liquor amnii."

This apparent exhaustion of the uterus may also depend on an engrossed state of this organ. This state of the uterus may be known by the labour commencing and proceeding, for a time, regularly and properly; but the uterine powers becoming afterwards gradually weaker, until they cease to produce any propulsive effects on the child. "The os uteri is disposed to dilate; but the presenting part is not protruded during pain, and this pain is felt over the whole abdomen. The woman complains of a sense of suffocation or sinking; the pulse is hard, full, or depressed; and the pains are irregular, both in force and frequency. *This case is relieved by bloodletting alone*, so far as we have yet observed." The author details the following case in point:

"Mrs. ———, aged twenty-eight years, in labour with her first child; her pains commenced regularly, and pretty severely for some time, and then became desultory in frequency and force. The midwife, before we saw her, gave her some stimulating drinks, which increased her unpleasant feelings, such as a sense of suffocation, heat, and pain over the whole abdomen, sickness at stomach, &c. without augmenting the force and frequency of her pains. When we first saw her, she was labouring under all the distressing symptoms just mentioned, together with a depressed pulse, frequent sighing, great uneasiness and apprehension, the mouth of the uterus not much dilated, though quite unresisting when attempted to be stretch-

ed; its edges were thickened, but not tense; when in pain, very little impression was made upon the child, and the mouth of the uterus rather contracted than opened. She was bled, at first to the extent of about twenty ounces; but as she was only partially relieved, and as the pulse acquired vigour by the operation, the bleeding was continued until all the unpleasant symptoms were subdued, which happened upon the loss of about ten or twelve ounces more. The pains now increased very much, and in about twenty minutes more she was safely delivered."

There is another variety of exhaustion of the uterus, which it is of importance to understand. In this the labour commences briskly, the pains returning quickly, and with considerable force. They are attended with the usual favourable precursors of labour, such as a subsidence of the abdominal tumour, and the secretion of mucous.

"After all these favourable appearances have continued a longer or shorter time, the *pains cease altogether*, or nearly so, without any evident cause, and the whole labour seems to be at an entire stand. The pulse is very little disturbed, but a train of nervous symptoms supervene, such as palpitation of the heart, great oppression about the præcordia, with a sense of suffocation if the patient attempt to lie down, and a disposition to syncope if she rise up. The os uteri is well dilated, and the membranes remain entire. This condition of the uterus is peculiar, and requires the administration of such remedies as may have a specific action upon its fibres, such as the secale cornutum, and is one of the happiest cases for the illustration of its powers. Stimulating medicines or liquors are decidedly injurious."

The directions laid down by our author, for "*conducting a preternatural labour*,"—for performing the operation "*of turning in each particular case of head presentation*," and "*for the use of the forceps*," are precise and satisfactory, and may be studied, with instruction, by the old and the young, the experienced and the inexperienced. It is in this part of the work, particularly, we think, that Dr. Dewees has shown himself the experienced and enlightened accoucheur.

His chapter on "*Rigidity, &c. of the soft parts as a cause of preternatural labour*," is one, also, of much interest and value.

When there is much rigidity of the neck of the uterus, the

soft parts yield with great reluctance, and make the labour extremely painful and tedious. In cases of this kind, the most effectual and certain mode of disposing the parts to relax properly, is to bleed copiously, so as to produce an approach to fainting. The author introduces several cases illustrative of the beneficial consequences of this practice,—a practice which we have ourselves, in a few instances, employed with the happiest effect. To exemplify, more particularly, the effects of copious bleeding in difficulty of labour from this cause, we quote the two following cases:

1. "Miss V. aged fourteen years and a-half, was taken in labour, January 14th, 1790. She had been in pain thirty-six hours before we saw her; that is, she complained for that period, though the pains were not very severe; about twelve hours before we visited her, the waters were discharged; the mouth of the uterus was but very little opened, and the external parts not favourably disposed; the pains now were very severe, and the head was pressed pretty deep into the pelvis; she was extremely costive, and had passed no urine for many hours; an injection was ordered, which operated very freely; the catheter was introduced, and nearly a quart of water was drawn off;—she was much relieved by these discharges. An hour was given, in hope that a favourable change might take place in her labour. There was but very little heat in the vagina, for she had been rarely touched. She had, however, by the advice of the midwife, been placed over water, and fumigated with burning onion shells, but to no purpose.

"We now bled her fifteen ounces; this produced some little change in the mouth of the uterus, but not sufficient to permit the head to pass, as it contracted and stiffened with each pain. In an hour more she was again bled fifteen ounces; this produced sickness of the stomach, which was our signal for stopping. Upon examination now, the parts were found sufficiently dilated; there was a temporary suspension of pain; but they soon returned, and were of competent force, and much more tolerable; the labour was soon after terminated."

2. "February 17th, Mrs. ———, aged forty, in labour with her first child. She had been long in labour previously to our seeing her, and had suffered much pain,—her pains were in quick succession; the waters were still undischarged; the uterus opened to about the size of a quarter dollar; its edges very firm; no disposition in the external parts to relax. She was bled largely, (forty ounces) and was delivered in half an hour after."

The same mode of proceeding, according to the experience of Dr. Dewees, is equally effectual in cases where the soft

parts have become contracted, or formed adhesions, in consequence of some local injury sustained from the long continued pressure of the child's head in a previous labour, or from lacerated perinæum, or finally, from the "ill-judged use, and worse directed application, of instruments." Several highly interesting cases are given, in which obstacles of this kind were safely and speedily overcome by bleeding, *ad deliquium animi.*

"On the 26th of September, 1820," says Dr. D., "we were called to a woman in labour, in consultation with Dr. Ruan. She had been twelve or fourteen months in travail with her second child.* The pains were frequent and strong, the waters had been discharged some time; the head was favourably situated, and completely occupied the vagina; the perinæal tumour was large; the os externum did not exceed in size a finger ring, admitting the finger with some difficulty, in the absence of pain; during a pain it would be thrown up against the inferior edge of the symphysis pubis, so as not to admit the finger, or permit it to remain when it had been previously introduced. Externally, a large cicatrix was found to run to the very edge of the anus; internally, it could be traced farther. This seam prevented the unfolding of the external parts so effectually, that the repeated efforts of the uterus for several hours, were insufficient to make them yield, though the head had been closely applied to them for that period. The patient was a strong, healthy woman; considerable fever was excited; the pulse was strong, frequent, and hard. We proposed bleeding, ad deliquium, to which Dr. Ruan consented. A vein was opened immediately, and we took away about forty ounces of blood; but as her pains were very rapid, we were obliged to draw it from her while in a recumbent posture, and no disposition to syncope was manifested. This quantity, however, had some effect, as there was evidently a disposition in the parts to relax, and an abatement of the severity and frequency of the pains. A second bleeding was determined on, and to perform it while in an erect position. We effected this with some difficulty; but upon taking five and twenty or thirty ounces more, she fainted;—she was laid on the bed, and was in a few minutes delivered by the forceps, of a fine healthy boy. The patient recovered rapidly, without accident or draw-back. About three years we again delivered the same woman, by precisely the same means."

The chapter on *Uterine Hæmorrhage*, contains a well digested account of the most authentic information contained in

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* With the first she had suffered an extensive laceration of the perinæum.

the books on this subject, interspersed with valuable original observations and facts, drawn from the author's own experience. We are particularly pleased with his remarks on the consequences which are apt to follow the retention of the placenta, after the expulsion of the fœtus in abortion, and with his mode of effecting its removal.

"In such cases," he says, "we have employed, with the most entire success, a small wire crotchet to bring it (the placenta) away. The manner of using it is as follows: the fore-finger of the left hand is placed within, or at the edge of the os tincæ; with the right we conduct the hooked extremity of the wire crotchet along the finger, until it is within the uterus, it is gently carried up to the fundus, and then slowly drawn downwards, which makes its curved point fix in the placenta; when thus engaged, it is gradually withdrawn, and the placenta with it. The discharge instantly ceases in every case where we have had occasion to use it. In every instance to which we here refer, we are persuaded that it preserved the woman."

On the subject of puerperal convulsions, Dr. Dewees' observations are very valuable. We regret that we have not room to enter upon a particular notice of his opinions and practice on this important subject. We trust, however, that there are few practitioners of midwifery in this country, who will not take an early opportunity of procuring and reading the work itself. E.

ART. VI.—*Reply to the Objections of the Medical Review, No. II. Art. IV. against Dr. Meigs' Lecture on the State of the Blood-vessels in Fever.*

Queris Ulysses ubi erraverit, potiusquam efficias ne nos semper erremus.
Seneca.

PEOPLE are generally both more fond and more capable of pointing out the errors of others, than of showing the true path of wisdom themselves; and this is one reason why the literary public has, in latter years, been favoured by the establishment of what are called Reviews; which being mostly conducted by eminent scholars, are received as nearly oracular in their commendations or denouncements of the several mat-

ters they take in hand; because these reviewers, in assuming to themselves the office of judges in *foro litterarum*, do, virtually at least, announce themselves as prepared by deep erudition to show in matters of science, not only Ulysses ubi erraverit, but what Seneca thought to be of far greater consequence, ne nos semper erremus.

A good review is then matter of thankfulness to the generality of readers, who, willing to take opinions at second hand, are thereby protected against all that loss of patience and waste of time, which would be incurred by the perusal of all the lucubrations that pour from the prolific sources of the press. A good review is characterised by a cautiousness which never commits itself; it never thinks it is shedding rivers of blood from the veins of misbelieving enchanters, while it really is assaulting the wine-skins from which it might have been nourished and enlivened; and why? Because it always sees with its eyes open, and never pretends to see in the dark; being aware of the great difficulty of seeing well, even in the best lights of learning. It generally admits all self-evident propositions;—it does not deny, at least, what is matter both of common credence and unequivocal demonstrability.—It knows that every reviewer, how didactical soever, is not Pythagoras, nor every reader his pupil, with the everlasting, ever-ready, magister dixit at his tongue's end; and is also aware, that among possible events, a reader might desire to peruse for his own satisfaction, an article which, passing unscathed under the sharp edge of his wit, had also come off unbruised by the bludgeon of his judgment and erudition. For in such a predicament, if a reader of Art. IV. should inquire where is Dr. M's. lecture to be found, he would be astonished to learn, that it had actually crept into a journal so respectable as that of the Medical and Physical Sciences, No. 14, edited by Professor Chapman. If Dr. Meigs', therefore, complaining of the unsatisfactory exposition of his theory contained in the review, should have preferred to leave the decision on its merits to readers in general, how polite

would it not have been to indicate to those at a distance, that the lecture had actually been printed, nay, was even extant.

We do not know that Dr. M. has any reason to complain of the temper of the review; for he is aware that those who communicate false doctrines, or make ridiculous applications of true facts in our important science, deserve ridicule, if not worse, reprehension. In the like good temper with that of the review, therefore, we shall proceed to enforce his doctrine by some additional arguments for its reasonableness, but more especially by showing, that the reviewer has not been able to refute those formerly advanced in Dr. Chapman's journal.

As the doctrine contained in the lecture, was of a character altogether novel, its readers were specially requested to examine it carefully, and, says Art. IV., "we feel ourselves invited to the task, &c." nevertheless, it seems he did not *accept* the invitation, for if any one should now ask him what is Dr. M's. theory, he would reply, it is this:—the *blood-vessels are weaker in the hot stage of fever than in health;* an idea of the theory which is not to be found in the lecture, so little attention has he paid to an essay he has undertaken to review, and to discredit.

He has not stated, or, perhaps, examined, Dr. M's. arguments at page 294-95, drawn from the antagonist character of all the operations carried on by vital contractions in the human body, without refuting which, he could not deny any of the inferences from them: he has totally neglected to show what are the true powers of arteries, or capillary vessels; and what is the nature of their action, or that of the heart.

From some of his assertions one would think he had neglected the very text-books of physiology, and he has certainly misunderstood parts of those he has quoted. This being the case, how can he say, we accept the invitation of the lecturer, since he has not qualified himself by careful study of the lecture, and other disquisitions, to appreciate the theory of Dr. M.

We shall quote here a part of the definition of the word "action," and refer for a fuller account of it to the lecture, or to the Dict. des Sciences, Med., where it is elaborately stated.

"When, therefore, I use the term action, in relating a supposed physiological or pathological condition of the heart, of an artery, or a vein, I do not use it to express the whole of that great vital function, the circulation of the blood, but only as hypothetic of the state of a single fibre, or all the fibres of the heart, of an artery, or a vein, or all the arteries, or all the veins. When I say action, I mean action, when I say function, I mean the function." p. 291.

Can a valid objection against this acceptation of the term be made? The reviewer acknowledges at page 217, that "if increased contraction alone constituted increased action of the heart, the proposition of the lecturer would necessarily be admitted." Now, if the whole of the doctrine which he was assailing, rested on this single point, it was his manifest duty, instead of rambling off after Bichat's untenable theory of the extension of organic muscles, to show wherefore it is, that contraction alone does not constitute action of the heart. The truth is, he has no reason for his objection, and I shall leave the matter here for the present, only with a reference to the article Action, in the Dict. des Sci. Med., where the physiological term is well defined.

To show how careless the reviewer has been in preparing his refutation, let us contrast the following sentences, in which, after charging Dr. M. with holding "incorrect views concerning the properties of arterial tubes," he announces, at p. 214, "Had he said, that in the cold stage of fever the arteries are more contracted than in the hot stage, he would have spoken correctly," whereby he tells us that Dr. M. has *not* said they are "more contracted." Again, "according to his notion, increased action of the heart means nothing more than *increased contraction;*" whereby he admits what the other sentence does not admit, viz: that the lecturer sup-

poses them more contracted in the cold stage, and therefore has "spoken correctly," as the review both does *admit* and does not *admit*. Now, as Dr. M's. paper was written to prove them more contracted, or more active in the cold stage, the foregoing will serve to show, that the reviewer did not read the paper seriatim, as a reviewer should do, or it will at least manifest the want of carefulness to which he felt invited: hence, Dr. M. has a plain right to appeal from the judgment of the reviewer, on this head alone: he has a much more just reason to appeal on the ground of want of clearness, in the exposition of his theory, and the arguments for it.

At page 293, the lecturer notices Bichat's arrangement of the systems of the body, and says,

"We find, as, for example, in the mucous tissue, a great uniformity of structure, properties, and application, from the knowledge of which (uniformity) we may draw the most important practical lessons; so, also, in the muscular, serous, &c. These are all governed by particular laws, which pervade every part of them, the law of one part being the law of every part of the same kind. Wherefore shall we not admit the same unity of properties in the circulating system? Have we not in our pathological reasonings, lost sight of the advantages to be derived from Bichat's arrangement, and separated too widely our ideas of the venous tissues, from those we have of the arterial and capillary, and all of them to an infinite distance from our ordinary notions of the heart? The heart and vessels constitute one single system, the heart is part of the circulating tissues,—it is an essential portion of that system of tissues to which it is attached and subservient, enjoying the same *kind* of vitality, dependent on the same sort of (ganglionic) nerves, therefore governed by the same laws, susceptible of the same exaltations and diminutions of actions with them."

On the next page is a reference to Bichat, for the circumstances which characterise different portions of this *system*, which the lecturer is, nevertheless, disposed to consider as an unit. We would now ask, with what face can the reviewer charge Dr. M. with sustaining this idea of the sanguiferous organs upon an "ipse dixit, he thinks so, and therefore it must be so," advancing no reason for this ridicule, ex-

cept that he does not *understand* what is meant by the unity spoken of, and which he yet proceeds to refute. Is there not then, evidently, a great lack of clearness and fairness in the exposition of Dr. M's. sentiments.

As Art. IV. did not condescend to point out the place where any reader might find Dr. M's. paper, in strict justice to that gentleman, he ought, in a clear and concise manner, to have stated Dr. M's. opinions, *with* some of his principal arguments.

May we not then here present an extract, in order to throw some light on those opinions.

"The *pathological* state of the veins is, I think, almost always referrible to a condition primary in the arteries.

"Both veins and arteries are constantly exercising a power, antagonist to that of the heart.

"In health they antagonise it perfectly,—in disease imperfectly,—either too much or too little.

"They of course antagonise each other. Both possess the power of diminishing their diameters, the length of which depends on the degree of antagonist force. Therefore, when they are small, it is because they antagonise with more force, more action; when they are large, because they do so with less force, less action.

"But they (the arteries) are small in a chill, and large in a fever.

"What do we see then in the large, round, full, pulse of fever, except the proofs of diminished resistance to antagonist power, and consequently the proofs of at least a relative debility." p. 295.

I will add of diminished action, and vice versa. The candid reader will obtain from the above extract a clearer idea of Dr. M's. sentiments than he can collect out of the whole Art. IV.

The arguments (if they deserve the name) at pp. 218-19, are only a series of sophisms, which can have not the least influence on one who takes the trouble to think, while he reads this physiological controversy. It is here, upon a strange misinterpretation of a plain and intelligible statement, that the reviewer builds up a most self-complacent refutation.

"How is this," says he, "the doctor has told us that in the hot stage the heart and arteries are in a state of (relative) debility, acting with diminished energy. If this be the case, how can the reduction of the mass of the blood, and an abstraction of stimulus restore the lost energy of the arteries, and terminate the hot stage? In the above paragraph, the author tells us, that in the sweating stage the momentum of the circulation is very great, —in this we agree with him perfectly, but how can he reconcile this fact with the principles of his doctrine. In the sweating stage, not only the arteries, but the veins, capillaries, and heart, are more dilated than in a state of health, and therefore, according to him, more debilitated."

We have quoted all this penetrative acumen, for the purpose of showing what the reviewer certainly regards as an inexpugnable position, but which, nevertheless, has clearly no manner of efficiency when compared with the actual sentiments of the paper; and if it has any appearance of strength or of appositeness, it lies in the erroneous supposition that Dr. M. has asserted the arteries, in the hot stage, to be in a state of actual debility, as compared with other portions of contractile tissues in the body,—and also in confounding the action of the vessel with its function, a distinction which the reviewer seems never to have made except by accident.

Let us put this matter in a clear light; Dr. M. has never stated other belief than the following: Marsh miasm, or the causes, whatever they be, of intermittent fevers, are of a stimulating nature. These stimuli excite the vascular system, the left, or arterial portion of which, is most easily excitable, or soonest capable of feeling the stimulation of the miasm; it *soonest* becomes more contracted, acts more powerfully, antagonises too much the right or venous portion of the system, and refusing to receive or contain the usual quantity of blood, accumulates it in the veins. Hence the small contracted pulse of chill. According to a known law of contractile and other tissues, excitement is soon followed by less excitement; therefore this excited, stimulated, active state of any artery, is followed by a state of less action or debility;—it is still, however, far greater than in health;—it is less, compared with its

antecedent amount, it is relatively weaker, because the veins which received the surplus in the cold stage, begins to restore the equilibrium of contractile phenomena in the sanguiferous system; they urge on the blood more rapidly to the heart, the cavities of which distended more completely, and yet contracting completely, throw out a larger column, making a larger pulse—throw it out frequently, making a frequent pulse—throw it out suddenly, making a quick pulse.

The action of the veins is here greater than in health, and we suppose was so even in the *congested* state, (with some exceptions) it was not *then*, however, equal to the action or power of the arteries. In the hot stage they are becoming balanced by the increasing exhaustion of the arteries, *i. e.* by what Dr. M. calls the *relative debility of those tubes.* In this balanced state, the whole sanguiferous system being equally pervious, and pressing more forcibly on the blood than it does in health, that fluid flies the round with great velocity.

This seems to us a very plain case. If we lay bare a long portion of artery in a living animal, it frequently contracts so much by the stimulus of air in dissection, as to obliterate its cavity. When Mr. Hunter divided the posterior tibialis in a dog, under such circumstances, the blood only oozed out from the cut end; will the reviewer charge this phenomenon on the debility of the artery? Will he call it diminished action,—a low pulse? Surely he ought, if consistent with his views.

I have seen an aged man die in the cold stage, I should say, of the cold stage of an intermittent; what is the real state of the case?

He shall have at six A. M., a good, round, tolerably full pulse,—not confined to bed: between six and nine o'clock, his pulse becomes more frequent and smaller,—at nine he is in the tremor of ague, his pulse is a thread,—it disappears entirely at the wrist, in the foot, at the bend of the arm, in the ham, and in a few hours he is dead,—why? Because, his ar-

teries and cutaneous capillaries have excluded a large proportion of the blood they ought to contain, and because the venous system of the head, thorax, and abdomen, are *consequently* actually gorged, and in a state of venous apoplexy so complete, that their great functions are interrupted.

He asks, "how can the momentum of blood be increased, when the force which moves it is diminished?" In the same spirit he might ask, how can convulsions take place in those who are dying of hæmorrhage?—We may suppose him to say, that convulsions can not be the penultimate symptoms of such persons, because just before death the weakness is great, but muscular strength is the expositor of force for the whole system; in convulsion, the muscles are strong, therefore there can be no convulsion;—or he might as reasonably deny, that a man walks better and faster in health than in convulsions, because his muscles are strongest in convulsions, or assert that a mill-wheel which is both drowned and has a great head of water on it, moves better than with its proper quantity above and below. If the heart is so much in action, so morbidly active that it will only give space for the reception of ℥ss of blood in its ventricle, it will surely not carry on the circulation as rapidly as if it will receive ℥ii at each diastole. The momentum, therefore, cannot be so great.

What is the meaning of momentum? The momentum is the quantity of matter multiplied into the velocity; the velocity has a relation to the amount of resistance,—the resistance is greatest in the cold stage. The moving force, that of the heart, being supposed the same. Dr. M. did not undertake to decide, nor can any one decide, the ratio of resistance, or velocity; suffice it, that the momentum is truly greater, because the resistance is less. Lastly, where has the paper stated the moving force to be diminished? No-where.

Having now made manifest such of Dr. M's. sentiments as might in vain be sought for in Art. IV., we think he will have an admitted right to appeal from the judgment of the

reviewer, on the ground of want of clearness and fairness in the exposition of his sentiments.

This being done, it will be lawful now to show some of the numerous mistakes which abound in Art. IV.—lawful, because on these much of his criticism depends,—lawful, furthermore, because in proving his errors we shall prove the soundness of the opinion we are upholding.

"The action of a circular fibre, as the author *correctly* observes, tends to lessen its diameter, and, on the contrary, its relaxation must increase its diameter." p. 213. In this truly Delphic sentence are contained the two following propositions:

1*st*, The action of a fibre tends to *shorten* that fibre, which is very true, and is all that can be said or supposed on the subject.—2*d*, The *action* of a fibre must *increase* its length. If any thing intrinsic to the fibre acts, it only shortens it,—very well: if any thing extrinsic acts, it is not the fibre that acts, but something else: "but," says he, "its relaxation must increase its length;"—why? Because the relaxation which he speaks of, is an active, vital, and intrinsic movement, according to him;—he even pretends that the heart can spring open with a wonderful force,—hence he thinks the diastole an *action* of a circular fibre, which "must increase its diameter,"—but has he not just praised Dr. M. for correctly observing that the systole is the only state of action?

We hope Dr. M. will never say the relaxation of a ring muscle, "must increase its diameter;" he never can say it very correctly, it would be incorrectly, for a muscle is not elongated as soon as relaxed;—it is relaxed whenever volition or stimulus cease to keep it in action. Nothing but an elongator can ever restore it to its greatest length. Elongators are required both for long and hollow muscles. Action depends on something intrinsic to a contractile fibre, relaxation is the cessation of action,—a negative elongation is effected in every case by something extrinsic, and cannot be its action, or any part of it. How completely then has he misapprehended the use of this physiological term.

Dr. M. supposes (p. 301,) that no straight or circular fibre of the body can physiologically lengthen itself, but only shorten itself. That the heart is a hollow muscle, whose only *action* is the contraction of its fibres, or diminution of its cavities;—that when most excited, most in action, it most resists the impulse of the veins, and receives, consequently, less blood,—in a smaller ventricle, and why smaller? Because, the hollow muscle excitable sooner, and sooner excited, discharges itself sooner,—is more active in throwing off its blood, —more in action. The arteries partake of this pathological condition. Can any one object to this? Or is action, in our sense, any thing more than that movement produced by the agency of excitants on excitable matter;—the action being greater in the ratio of the excitant, or the excitability, or both.

Says Art. IV., "were it true, as he supposes, that increased contraction alone constituted increased action of the heart, then, indeed, we should be obliged to admit that a small pulse is indicative of increased action, at least of the heart, if not of the arteries." And has not the reviewer admitted, as before quoted, that "the action of a circular fibre, as the author *correctly* observes, tends to lessen its diameter?" Now we are inclined to think, that if the action lessens diameter, increased action lessens it still more, or contracts it still more. We recommend some of the reviewer's "daylight," for the express benefit of this part of Art. IV.

Mr. Hunter, whose judgment in matters of physiology was nearly unquestionable, because of the great carefulness and coolness with which he judged, as well as because of his great genius and penetration, has made a comparison of the phenomena of the circulation, produced by *exercise* and by *disease*;—he says:

"And if a greater quantity (of blood) be thrown out, (from the heart) and with a greater velocity, then the arteries must *relax* in proportion,—since the different parts must correspond with each other. We must therefore

suppose, that in *health*, whenever there is any exertion greater than common, which always increases the pulse, the heart dilates more, and contracts more, and does both with greater velocity,—this arises from a necessity which I conceive begins first in the veins." p. 125.

Exercise of the muscles tends to urge forward the blood in the veins, causes it to arrive in large quantities at the heart, which being in a state of natural, not morbid excitability, allows its ventricles to be disturbed fully before it acts,—it takes in a large quantity by a perfect diastole, after which it acts, and lets out a large column into the vessels, which being also in a healthy condition, are ready for the increase of circulatory function. Hence the large artery from exercise; and in this case Mr. Hunter justly observes, that the necessity is primary in the veins. But, proceeds he,

"Does the same thing take place in disease? Does the heart dilate more, contract more, and also contract with more velocity? I believe this case does not in the least correspond with our former position. *The pulse, in such circumstances, although frequent, is small and hard, showing the arteries to be much contracted by their muscular power, and therefore unfit to receive a large quantity of blood from the heart in a given time.*" p. 125.

It would be difficult to give a clearer expression of Dr. M's. theory of the state of the blood-vessels in fever, than is contained in the above extracts from the treatise on the blood: we only object to the supposition, that the velocity is less, (*i. e.*) the contraction less sudden;—we apprehend the action is much more sudden.

The description of the phenomena of the heart and vessels, as arising from exercise, hardly differs from Dr. M's. account of them in the hot stage, between the phenomena of which, and those arising from exercise, there is often the strongest analogy,—nay, sometimes even identity. The lecture supposes the hot stage to depend on the increase of circulation, produced as in exercise, by the increased relative power or activity of the veins. The cold stage depends on the increased power or activity of the arteries, which, to use the

language of Mr. Hunter, are unfit to receive a (sufficiently) "large quantity of blood in a given time."

With what face, then, can the reviewer say, he has seldom witnessed a doctrine more void of solid foundation? Is the sanction of John Hunter's profound judgment of no value, or does it afford no semblance of strength to any argument? Will he show how it is not true, that contraction is the only *action* of the heart? Or does he now feel "obliged to admit, that a small pulse is indicative of increased action, at least of the heart, if not of the arteries?"

To show how our reviewer flounders in the sea of physiological doubts, on which he has embarked, take the following:

"The heart, however, is not passive in its dilatations,—the *action* of dilatation is not merely a remission from *action*, but as much the effect of *active* powers of the heart, as its contraction." p. 218.

"Yet action," as the author correctly observes, "tends to lessen its diameter."

The notion of an active diastole, which was exploded so long ago, he certainly would not have brought to prop up his sentence delivered against Dr. Meigs in the very outset of Art. IV., unless that heedlessness which distinguishes his review, had also attended his study of Bichat. Let him think as well as read;—if he had given one sober thought to Bichat's article on the phenomena of relaxation in organic muscles, he would have perceived that that great man was led by his passion for analysis and refining, in direct opposition to reason and fact; that each, and every one, of the six reasons advanced by him in support of his idle doctrine, is alike destitute of foundation; and finally, that the whole error has arisen from a careless performance of a very common experiment. For example, take two living hearts, thrust your finger into the ventricle of one, while in the other hand you grasp the other heart. "You will feel," says Bichat, "that the latter

makes as great an effort in dilating, as the other in contracting."

He goes so far as to say, that the effort of dilatation is "even sometimes greater than that of contraction."

It is most surprising that so acute a physiologist could have overlooked the real state of the case, as he has done, in mistaking for the diastole, what was in reality the systole of the heart. When he took it into his hand, it was in a state of relaxation, it was flaccid and compressible; when it contracted, it became suddenly shortened, and thickened in his hand, forced his grasp in some measure open, and gave to his careless observation the *appearance* of diastole, when it was the systole he was remarking.

Sprengel, in his Histoire de la Medicine, charges Wm. Harvey with holding this same doctrine of active dilatation: I have sought in vain for such an opinion in the writings of that sensible physiologist, certainly it is not in either of the Exercitationes Anatomicæ.

On the contrary, in that ad Riolanum, he says, "Pateat quod in ventriculos sanguis ingrediatur, non adtractione aut *extentione* cordis, sed pulsu auricularum *immissus.*" In another place he expressly corrects Cartesius, who had fallen into the same error with Bichat and our reviewer, of *mistaking* the systole for the diastolic state, and assigns the true cause of the latter, namely, the antagonist power of the auricle. 280. Harvey says again, "neque in diastole vigoratur cor, sed in systole;" again, "neque verum est quod vulgo creditur, cor proprio suo motu aut extensione sanguinem in ventriculo adtrahere, sed dum *movetur* et tenditur *expellere*, dum laxatur et concidit, recipere sanguinem." ap. Manget Biblioth.—I. 846.

Old Francis Glisson, perhaps the most profound and erudite investigator of early physiology, says, "Actio ergo Fibræ, sive motus activus, duplex est, contractio et relaxatio; sed hæc (relaxatio) minus proprie actio audit, *consistit enim in remissione actionis, et reditu ad nativam figuram.* Quare,

quando de *actione* fibræ indiscriminatim loquimur, semper intendimus *vigorationem* ejus et *tensionem* sive CONTRACTIONEM, non relaxationem," De Vent. et Intest. cap. v. One would think that this standard author gives abundant support to D. M's. acceptation of the term, and his application of it.

Haller says, that in relaxation, the heart is "leve planum et molle," Phys. I. 387. Also, "Post brevem systolen diuturnam diastolen succedere in qua cor *molle laxum et quietum* manet." There are many such places—he expressly refutes Pechlin.

Mr. John Hunter, looking at a heart during artificial respiration, observed, "that the ventricles were not turgid at the time of their diastole, for I could *feel* them *soft*, and could *easily compress* them."

Is it not then useless to pursue any further the proofs of our reviewer's error. They are every where abundant, and seem to be fixed almost intuitively in the mind of every student. How could he then so didactically announce "unquestionably, *therefore*, an increased action of the heart consists both in a more vigorous contraction, and (vigorous?) dilatation (qu! relaxation?) of this organ." 28.

We beg leave here to remind him of his promise to admit D. M's. theory "if it were true that increased contraction alone constituted increased action of the heart."

Let us now proceed with our plan of reply, by showing his errors, and therein upholding our own doctrine on the properties of arteries, concerning which, he gravely charges Dr. M. with holding "erroneous views." Here is an apposite dictum. "In short, their principle of contraction is totally distinct from that of the heart, being the consequence, not of an excitation of muscular contractility, but of an *elastic* power inherent in the arterial structure, by virtue of which these vessels contract and dilate, according as they are distended by mechanical force." It is hardly possible to suppose a reviewer unacquainted with Mr. Hunter's standard work—what then does he understand by the three states in which arteries

are found—vid., the natural pervious or middle state, in which it is placed by the elastic coat; the stretched state, or distended state; and the contracted state which it acquires by the action of its contractile fibres; fibres, which in vessels of moderate size, are able completely to obliterate the tube, as in Mr. Hunter's experiment on the posterior tibial artery of a dog; on the umbilical arteries, &c. This contractile coat is the only one of which physiological action can be predicated, and in which only it is observable. Are not Mr. Hunter's experiments conclusive, are not his remarks, from p. 90 to 108, absolute demonstration of the active, vital contractility, nay, I will say, even muscularity of the arteries? Parey's work is full of demonstrations on this head, he has often seen the artery contract on exposure; in one case it contracted as if a string had been drawn round it.—Exp. 18. We have no room to quote here more authorities—it is also not necessary, as the above may sufficiently reprove his hasty charge against Dr. M., of "holding erroneous views," &c.

We have striven, but cannot resist the temptation to expose him in the following:

"The principle of contraction is an elastic," &c. &c. "This elasticity is what Bichat calls the contractility of texture." p. 215. What Bichat calls the contractility of texture!! "L'elasticitè des artères," says that physiologist "est aussi remarquable apres la mort que pendant la vie; il est *essentiel de la bien distinguer de la contractilite de tissu.*" Among many reasons for this distinction, he says "3d. tout mouvement elastique est brusque, soudain, aussi prompt à cesser qu' etre produit, au contraire, tout mouvement de contractilitè de tissu est insensible, lent, dure, souvent plusieurs jours."—Anat. Gen. I. 390—91. He also calls it contractilite par defaut d'extension. It is Francis Glisson's perceptio naturalis; if it is not Haller's vis insita, I know not where to place it in the system of that author, for it is surely neither the vis in genere, mortua, or nervosa, of that philosopher; it is muscu-

lar action, produced by the stimulus of necessity of John Hunter.

We confidently hope the reviewer will now give up his reliance on that elasticity which "Bichat calls contractility of texture," and of course give up the idea of a refutation to be founded on such "erroneous views concerning the properties of arterial tubes." We might also here address him with the old apophthegm, de te mutato, &c.

Has the author of Art. IV. ever seen the transverse section of an artery? Has it not always presented the appearance of a ring? This ring is no longer or smaller than the unobstructed elasticity of its "*membrane propre*," which we call its elastic coat, would make it: if you press it flat, its elasticity restores it to its circular state, just as a ring from a bottle of couatchoue would, by a physical force, totally unconnected with vitality, or its phenomenon action. If the ring was even larger during life, it was made larger by the injecting, distending force of the heart's systole; if smaller, it was because of the *action* of its contractile coat, not of physical elasticity, which keeps it open,—or in the middle state. It is by the combined operation of its elastic coat and contractile fibres, that it resists or antagonises the heart in ordinary health: but in disease, where the heart is stronger, or the vessel weaker, we have a large pulse, the antagonisation being dependent more entirely on the elastic coat—but if the vessel is stronger by some morbific stimulus, its contractile fibres operate, both against the heart's force, and the elastic coat, which latter it renders within or below the middle state. In this manner it may antagonise the heart; and by thus resisting the intromission of a large column of blood, give us a small pulse from increased action of the vessels.

We will now proceed to the exposure of more of our reviewer's errors. They abound in his disquisition on the capillary vessels, of which at p. 216, he says, "their dilatation and contraction depends on a principle altogether different from that which determines the same changes in the arterial

tubes."—Again, "They dilate or contract altogether independent of the heart or arteries. In inflammation, for instance, they dilate, either because they are debilitated, or because their organic sensibility, according to the ingenious theory of Bichat, is changed." &c. &c.

Now, if a circular contractile fibre can only shorten and not lengthen the diameter of a vessel, what becomes of his surpassing assurance that "their dilatation is altogether independent of the heart;" the answer is plain, he is mistaken q. e. d. And further, to "the ingenious theory of Bichat," the ingenuity of which the reviewer seems not to have apprehended, is it not evident, nay, strikingly so, that any change of organic sensibility of capillaries producing or permitting dilatation of them, can be only a change—from what? from the strength which can resist, to the weakness which can not resist the *intromission* of red globules, which are therefore forced into them by the vis a tergo, so unwisely rejected by Bichat and his hopeful pupil, our reviewer, after him, vid. Anat. Gen. I. 26. et. seq. Let us not be gainsaid, as here mistaking sensibility for contractility, we are not so misapprehensive; an increased sensibility must tend to *increase* the aptitude of the capillaries to exclude, not to *diminish* that aptitude.

Our readers will perceive then, that the "ingenious theory" does not save him. By a slight analysis, it is reduced to the true doctrine of dilatation from debility, actual or relative, the heart being the antagonist or elongator; the simple reading of the passage is its reductio ad absurdum: and we cannot but smile at the solemn annunciation, that the capillaries, which "*act* by an inherent principle of *contractility*," 216, are, "in every inflammation, *dilated*, and, if you please, *debilitated*," 217; while "in fever, on the *contrary*, the arteries are enlarged, not by their own *action*, but by the larger quantity of blood *forced* into them, &c. &c." 218. Now, if dilatation follows weakness, why does it follow it? Because, as before said, there is forcible injection by the action of the heart; how

marvellously quidnunc does the "on the contrary" therefore appear.

As to the reviewer's remarks on the practice resulting from the doctrine of the lecture, we shall observe that they are the erring products of his misunderstanding of Dr. M's. paper, attributable, we presume, entirely to careless reading thereof, so manifest throughout the IVth Article. Had he given a little attention to a paper he has been willing so utterly to condemn as ridiculous, he would have seen the theory of the author is simply this: in fever, the *system of sanguiferous tissues* is directly stimulated, instead of being debilitated, as is generally supposed, by the miasmata of marshes. This system being an unit of tissues like as the mucous system, yet is characterised by some circumstances of difference in different parts, all however possessing contractility, and able, during health, to antagonize each other so justly, that no morbid accumulation of blood shall take place any where; but able, under other conditions of stimulation or disease, to act or contract in an undue and morbid degree, and thereby destroy the regular proportion of the healthy and balanced circulation.

The vessels to the left of the heart, (divided according to Bichat's view,) are very excitable;—easier thrown into excessive or morbid action, wherein they refuse their regular proportion of function, when they act most violently, causing the greatest interruption of the function. In such cases we have little blood in them, and much in the veins, a venous congestion or engorgement, a chill. The veins, by a gradual and steady resistance, tend to overcome this disproportion; they are able to effect it as soon as the arteries, in consequence of the necessary exhaustion of their superior contractile force, come to be on a level in this respect with other portions of the sanguiferous system.—This contraction (action) of the veins, urges the blood more rapidly towards the heart, which in a given time takes in more, and throws out more than in the common healthy state, and far more than in the antecedent chill stage; so as that the congestion gradually dis-

appears; the circulation becomes more free and unobstructed, a great development of animal heat takes place; it is the hot stage. Dr. M., with great propriety, maintains, that a full pulse here argues debility and decreased action of arteries, "relatively to an anterior state of those tubes," as he says; since their state of dilatation is the effect of a mere mechanical distention, pushed so far exactly, as the vital contractile fibres of the artery and its elastic coat will permit, the dilatation being the inverse measure of *action*, the direct measure of weakness.

If we might suppose a sentence from Mr. Hunter, it would be, the phenomena of the cold stage are primary in the arteries—those of the hot stage are "primary in the veins."

Physicians rarely sit more than fifteen or twenty minutes by the bedside of the patient. They have not, therefore, examined closely the appearances of chill, which last sometimes two hours. Those who will examine the state of the pulse in ague every five or ten minutes throughout, will be apt to come over to our opinion.

On the 23d Nov. at Magdal. Asylum, I saw C. Smith, æt. about twenty-eight—has had several paroxysms of intermittent—the fit had lasted fifteen minutes when I called. My watch had a quarter second hand: at four minutes before

	12	pulse 108.
past	12	128. Small, sharp, hard or corded, she is very chilly; shakes.
	12	136. Smaller, hard, she shakes, teeth chatter; ears, nose, and fingers cold.
15	12	128. Pulse not easily compressible, very quick; shaking and rattling of teeth together.
18	12	136.
20	12	140. Shakes violently.
22	12	140. Head begins to ache, says her back has ached sometime; has now some sensations of returning warmth, yet shakes violently with ague; teeth chatter.

25 past 12 pulse 140. Pulse very small, yet hard, not *soft*, nor easily compressed; has nausea.

30 12 124. Still shaking; pulse seems to be larger; heat of the body begins to increase.

35 12 128. Not so much shaking.

40 12 148. Shakes more than ever; her chill, which appeared to be diminished, seems to have returned with more intenseness.

45 12 120. The heat of the body more considerable; has head-ache and nausea. As I wrote the last remark she began to vomit.

I now left her with particular request that the matron would inform me of the period at which the chill might be considered as gone.—The next day I was informed that it ceased at one o'clock, (i. e.) in about fifteen minutes.

To return. We see no inconsistency in the theory, as applied to hectic and typhus.

Hectic is emphatically a disease of the arteries, and to consist with Dr. M's. idea, should be marked by a small and frequent pulse: Is it not so marked?

What is the relative condition of the right and left portions of the sanguiferous system in typhus? Is not there a preponderance of that fluid in the veins? But Dr. M. has not been obliged to admit that the arteries act violently in typhus, much less in syncope. In the latter case he believes they *sometimes do*, and thus produce that phenomenon—not always, nor often. He is aware that the heart may be really debilitated—that it may entirely cease from acting, as in some cases of syncope, in which the heart, not properly susceptible to the stimulus of the blood, may receive two or three ounces, and not throw out two drachms; the artery, however, would close on the two drachms as well as on the two ounces—because its contractile fibres constantly tend to the utmost contracted state of Mr. Hunter. Dr. M., therefore, may explain syncope on the ground of weakness of the heart, and consistently

show the small pulse of that disease to depend, not on an *actual* increase of arterial strength, but on a diminution of the elongating or impulsive effort of the heart, the veins remaining, as to action, unaffected.—Here the reviewer has a fund of arguments against Dr. M., and it was only from arguments collected here, that he expected to see his doctrine attacked, and perhaps overthrown.—It is not inapposite here to repeat, that the lecture was "on the state of the blood-vessels in fever," not on syncope.

The theory teaches us to avoid bleeding in the cold stage. "In our humble opinion" says the IVth article, "the true tendency of this doctrine is to lead to, rather than deter from the adoption of such a practice."

The opinion may be truly "humble" as he says, and we will not gainsay him; especially as we have shown so many of his errors in physiology and pathology. The therapeutics is, that as in the cold stage the arteries are in a state of violent action, the loss of blood will not have the least tendency to alter the *relative* conditions of the right and left portions, or the systems of red and black blood, as they are denominated by the "ingenious Bichat." If one were mad enough to bleed in this stage, would he lessen the quantity of blood in the veins, and not also in the arteries?

Moreover, what he calls "congestive forms of fever" are not cold stages of intermittents, though analogous to them. In their commencement, when the disproportion of action is not great, they bear bleeding—the practice is rash in more advanced states. This doctrine teaches us not to give brandy and red pepper in pleurisy, &c. "Why not," says Art. IV. "if he is true to his theory, are not these articles stimulants? —yea verily; but least they should act as such on the already stimulated vessels, and not by some "ingenious theory," select the veins for their special impression, we will have none of them, until the softness of the pulse and its volume give reason to believe, that debility in the whole sanguiferous system, and other parts, requires their use.

Furthermore, we deny that the lecture any where hath it that a "round, full, and *hard* pulse is indicative of arterial debility" 221, (i. e.) actual debility. Dr. M. observes, that the pulse, noticed just before the chill, is sometimes characterised by hardness, as is natural when the vessel begins to antagonise too powerfully the dilating effort of the heart, and resisting effort of the capillaries and veins.

Finally, we appeal from the judgment of this reviewer, and disclaim, for Dr. Meigs, such "daylight" as he has shed down in Art. IV. We close, by advising him to study the Physiologists, and commend to his special attention, that passage of the Anatomie Generale, in which the "ingenious" Bichat says, this *elasticity* is what I call "contractility of texture."

M.

Remarks on the preceding Reply. By the Reviewer.

That Dr. M. should not relish our strictures on his *theory*, is quite natural. In this we see but the influence of that generous impulse, which urges all beings to cherish and defend their offspring, however feeble or deformed they may be. We have no reason to be displeased with this trait of good feeling in any one,—much less in one who is so delighted with his little darling, as to fancy it of sufficient consequence to be committed to the fostering care of one of the first physiologists of the age.* It is doubtful, however, whether the Doctor has acted wisely in bringing it again into the field. If he had reflected on its weak and sinking condition, he would not have exposed it to unnecessary danger, but kindly administered enthanasial remedies, and suffered it to die in peace.

* The Doctor has dedicated his "Lecture" to Wilson Philip. Should this gentleman, through kindness, take it under his protection, he will find himself much in the situation of the nurse, in these lines of the poet:

"The tender infant, meek and mild,
Fell down upon a stone;
The nurse took up the squeaking child,
But still the child squeaked on."

The doctor, however, is not merely on the defensive in the above reply. He has advanced upon us right bravely, and seems to be exceedingly pleased with his success. It may nevertheless be well to show that his blows, though aimed with great deliberation, do not take the effect which one might suppose from the earnestness and apparent air of triumph with which he deals them out.

To show that we did not read his paper carefully and "seriatim, as a reviewer should do," Dr. M. endeavours to fix upon us a contradiction, which, as it is not to be found in our review, must be referred to his own imagination. He thinks that we are guilty of a great contradiction in saying at one place, that he ought to have said the *arteries* "are more contracted;" and at another, that according to him, "increased action of the *heart* means nothing more than increased contraction." We will quote, from our review, the two paragraphs to which his shrewd criticism refers: "His first and fundamental position, that in the cold stage of fever the *arteries* contract with *more force* than in the hot stage, is erroneous. Had he said that in the cold stage of fever the *arteries* are *more contracted,* than in the hot stage, he would have spoken correctly." Again, when speaking of the action of the *heart,* we say: "According to his notions, increased action of the *heart* means nothing else than *increased contraction.*" In the former of these two passages, therefore, we state what, in our opinion, he *ought* to have *said,* in relation to the *arteries;* and in the latter, what his *notions are* concerning the action of the *heart.* The doctor's critical dexterity must be much admired if he can show any contradiction in this.

The doctor charges us of not having been "clear and fair" in our account of his theory. If we have been deficient in this respect, we are sorry for it; for we certainly intended to be both clear and fair in our exposition of his doctrine. We are not conscious, however, of having failed in our duty on this head, and we therefore leave this point for the decision

of the reader, after having read our review, and the doctor's own "exposition," in the above reply. His object in this exposition, (page 411,) is to show, that the circulating system is an *unit* of tissues, enjoying *in all its parts* the *same kind* of vitality. He asserts that we do "not *understand* what is meant by the *unity* here spoken of." If the doctor has wrapt up some very profound and occult meaning in his phraseology, we own that we have not yet succeeded in getting at it; but if he means what he says, which may admit of some doubt, we do think that we know what his "unity of the circulating system" means. The doctor's reasoning in support of his position, that the circulating system is an unit of tissues, and that it enjoys the same "kind of vitality" in all its parts, is at once vague and illogical. "We find," he says, "as, for example, in the mucous tissue, a *great uniformity of structure*, properties, and application, so also in the muscular, serous, &c. These are all governed by particular laws, which pervade every part of them, the law of one part being the law of every part of the same kind. Wherefore shall we not admit the same unity of properties in the circulating system?" This argument rests upon the assumption that the circulating system possesses, like the mucous tissue, "*a great uniformity of structure*," which is by no means the case. The mucous tissue may with truth be said to be an "unit" in relation to its structure; but this cannot be said with equal correctness of the sanguiferous system. The heart differs very widely from the arteries in its tissues, and these again from the veins. The heart is almost entirely composed of muscular substance, whilst it is still a matter of dispute, whether the arteries contain any portion of this structure whatever, except, perhaps, in the aorta near the heart, and in the capillaries. "The proper coat of the arteries," says Bichat, "is exclusively peculiar to them, possessing no relation to the other systems, but forming a distinct and separate texture in the animal economy." (Anal. Gen. V. I. p. 307.) To make

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out his argument, the doctor observes: "the heart and vessels constitute one single system, the heart is part of the circulating tissues,—it is an essential portion of that system of tissues to which it is attached and subservient, enjoying the same *kind* of vitality, dependent on the same sort of (ganglionic) nerves, therefore governed by the same laws, susceptible of the same exaltations and diminutions of action with them." Dr. M., no doubt, feels perfectly satisfied with this reasoning, but he must excuse us for considering it exceedingly futile. That the heart and vessels constitute "a single system," when viewed in a general way, we can readily admit; but that they form a "single system," when considered in relation to their structure and particular functions, we are by no means disposed to allow. The heart, arteries, capillaries, and veins, all co-operate in circulating the blood; but these distinct sections of the general sanguiferous system, perform very different parts in this important function. While the heart acts upon the blood, as a propelling power, the larger vessels serve, perhaps solely, as conduits for its conveyance. The position that these parts "all enjoy the same *kind* of vitality," is a mere assertion, and has, in fact, no intelligible meaning. The doctor seems, indeed, to be fully aware of the above objections to his doctrine. In his lecture he says: "I would not have you suppose me ignorant of many circumstances of difference, in different portions of the vascular system. Bichat has pointed them out,—but *they cannot prevent me* from considering the whole as an unit of tissues." We are perfectly satisfied that nothing could prevent him from adhering to his opinion. His theory absolutely requires him to be pertinacious on this point; hæc mea sunt, teneo.

The doctor considers our remarks on his explanation of the sweating stage of fever, as a good specimen of "*penetrative* acumen." He must be wrong; for we had not the remotest idea of exercising any "*penetrative* acumen," upon so *superficial* a thing. It may be well, however, to show

that with whatever "penetrative acumen" we may have examined his doctrine, our remarks on its errors have not suffered much from his observations. According to the doctor's theory, miasmata stimulates the heart and arteries,—these contract, and push the blood into the veins, and this forms the cold stage. "If the arteries, in consequence of their *increased* action in the *cold* stage, should have their energy reduced by exhaustion to a level with the veins, or should the veins, by the stimulus of distention, recover their superiority, we shall next perceive the evidences of their reaction," that is, the supervention of the *hot stage*. "But what is the third stage? I answer," says Dr. M., "that the natural termination of the foregoing condition, is to be looked for in some evacuation, which, by diminishing the quantum of fluids, and removing irritating recrementitial particles retained in the former, may reduce the mass, momentum, and stimulus." Upon this passage we remarked in our review: "the doctor has told us, that during the hot stage, the heart and arteries are in a state of debility, acting with diminished energy. If this be the case, how can a reduction of the mass of the blood, and an abstraction of stimulus, restore the lost energy of the arteries, and terminate the hot stage." In order to get over the evident contradiction involved in the passage to which these remarks apply, he tells us in the present "Reply," that although the action of the *heart and arteries* is much *less* in the hot than in the *cold* stage, yet "it is still *far greater* than that which exists during perfect life." And hence he would infer, in contravention of our objection, that the reduction of the mass of the blood, and the abstraction of stimulus, is necessary to bring their action down to a healthy standard. We ourselves believe, that the action of the heart and arteries is *far* greater in the hot stage of a fever, than in health, and that the reduction of the mass of the blood, and removal of stimulus, is necessary to bring them down; but we perceive in this admission, a palpable inconsistency with the spirit and true import of our author's reasoning and principles,

and it was with a view to expose this inconsistency, that we introduced the above passage into our review. In order, however, to show that Dr. M. has no right, according to his own principles, to assert that the *arteries* act with *far* greater force in the hot stage of fever, than in health, we need only refer to the reasonings he uses in his lecture.—He there tells us, that when an artery *acts*, it "diminishes its tube." That in proportion as any muscle *is stronger*, in a physiological sense, so will its *action be stronger.*" If, then, the *action* of an artery *diminishes its tube*, its *deficiency* of action must be attended by its *dilatation*, and this *dilatation*, cæteris paribus, must be in proportion to the degree of *deficiency* of the action. Now, if the action of the heart and arteries be *far greater* in the hot stage of fever, than in health, their tubes, according to the reasoning of Dr. M., should be *smaller* or more contracted than in health. But is this the fact? Who does not know that in almost every instance the pulse in the hot stage of an intermittent is more voluminous than in health? We may observe here, that this is a new *dictum.* In his lecture he does not say a word about the action of the arteries "being *far greater* in the hot stage of fever, than in health." He has found it convenient to adopt this idea, in the above reply, in order to disengage himself from the perplexity into which our objection threw him. He speaks, indeed, of the *relative* debility of the heart and arteries in fever; but in other places he maintains the broad and unqualified position, that the heart and arteries act with more force in the cold stage than in the hot; and that they are dilated and debilitated in fever.

The doctor's theory rests solely upon the two following positions: 1*st*, A small pulse depends on *increased action* of the heart and arteries. 2*d*, Marsh miasmata, in their primary operation, excite the vascular system to increased action. With regard to the first of these positions, its fallacy is easily shown. In approaching syncopy, the pulse is small; but will any one say that the heart acts more vigorously in this state? Bleeding or cold, or a nauseating dose of antimony, will produce the

same effect; yet we presume that even Dr. M. will not say that these causes increase the action of the heart and arteries. A *small* pulse is, therefore, no sure evidence of an increased action of the heart and arteries; and hence the doctor's position is not sufficiently established; for it rests entirely upon the testimony of this symptom, which, as it is fallacious, will afford it no certain support. The fact is simply this: If the heart be debilitated, its action becomes diminished; it sends out less blood into the arteries, and these, being endowed with an *elastic* power, by which they accommodate themselves to the quantity of the contained blood, assume a size corresponding to the quantity of blood which is driven into them by the heart.

The assertion that marsh miasmata stimulate the heart to increased action, as a primary effect, is not supported by a single fact. It is a mere *assumption*, predicated upon the previous assumption of the increased action of the heart, in the cold stage of fever; for having once determined that such is the case during the chill of a fever, he would of course ascribe a stimulating, and not a debilitating power to miasmata.

Having established, in his estimation, the correctness of the position, that the heart and arteries are in a state of *increased action* in the *cold* stage of fever, he could find no difficulty in raising the superstructure of his theory; for he had but to ascribe to them a subsequent debility, in consequence of their previous great action, and assume a contemporaneous increase in the action of the veins, for the purpose of pushing the blood into the debilitated arteries, and he had the hot stage. Now, although the state of dilatation of both the veins and the arteries in this stage, is generally *greater* than in *health;* and although, according to the author, dilatation is synonymous with relaxation, or *diminished* action, yet, the author finds it necessary, in order to put a consistent *sweating stage* to his fever, to assert that the action of the arteries and veins is, notwithstanding, greater than in health! For how

would it otherwise do to say, that the hot stage is put an end to by "the removal of irritating recrementitious matter and *stimulus*? The fact is, such a removal of recrementitious and stimulating matter does take place; and although the veins and arteries are more dilated than in health, and therefore, by his theory, more *debilitated*, yet our author found it necessary, upon second reflection, to ascribe to them great strength of action, in order to make his explanation of the third stage consistent, and obviate the objection we have already made on this point.

Dr. M. thinks we asked a very silly question in relation to his opinion concerning the momentum of the blood in the sweating stage of fever; and very kindly offers us some information as to the import of the word momentum. To show, however, that the question we asked, is a correct one, and such a one as Dr. M. cannot answer in consistency with his theory, we extract from our review the passage in question. Dr. Meigs', in his lecture, says, "the momentum of the circulation is now (in the third or sweating stage) very great, for the arterial, capillary, and venous systems, and the heart, are now equipoise; the whole system is pervious, being equally free for the passage of the blood, with a heart beating oftener and throwing out more blood than in a state of health." Upon this paragraph we observed: "how can the fact, that the momentum of the circulation is very great, in the sweating stage, be reconciled with the principles of our author's doctrine? In this stage, not only the arteries, but the veins, capillaries, and heart, are more dilated than in a *state of health*, and therefore, according to the reasoning of our author, more *debilitated*. How, we ask, can the momentum of the blood be increased, when the *force* which moves it is diminished?" Now, as the heart, arteries, and veins, are all in a greatly dilated state, their action, or propelling force on the blood, according to Dr. M., is diminished; and as the mass of the blood is diminished by the increased secretions, how can its momentum be increased when the power which moves it is also diminished? The doctor himself, with all

his sagacity, has been guilty of a palpable contradiction on this point; for he tells us in the same paragraph, that the momentum of the blood is both *reduced* and *increased* in the sweating stage of fever. The hot stage, he says, is put an end to by the evacuations which supervene in the sweating stage; "which, by diminishing the quantum of fluids, and removing irritating recrementitial particles, may *reduce* the mass, and *momentum*, and stimulus. The *momentum* of the circulation *is now very great, &c.*"

The author of the "Reply," has given us a cento of disingenuous quibbling on this observation in our review. "The action of a circular fibre, as Dr. M. correctly observes, tends to lessen its diameter, and, on the contrary, its *relaxation* must increase its diameter?" This sentence, says our shrewd theorist, contains two propositions: "1*st*, That the *action* of a fibre tends to shorten that fibre. 2*d*, That the *action* of a fibre must *increase* its length." This miserable perversion of our words and meaning, is scarcely worth an answer. Do we say that *relaxation* and *action* are convertible terms? If a circular fibre contract, and thereby shorten its diameter, common sense informs us that when it relaxes, that is, when the contraction ceases, its diameter must be elongated. When speaking of the action of the heart, which, by the by, is not composed of *circular* fibres, we adopted that side of the contested question in relation to its action, which alleges, that the dilatation of this organ is not a *relaxation*, but an active effort of its contractile structure. On this point, Dr. M. observes, "and he even pretends that the heart can spring open with wonderful force." We are not the only persons who pretend to think so. In this opinion we are supported by the authority of Bichat, Pechlin, and many other physiologists, of the highest reputation.

Dr. M. carps at our remark, "that increased action of the heart alone does not constitute increased action of this organ." If the testimony of Bichat and others, that the heart dilates by an *active effort*, is to be credited, then the action of this

organ, taken in its proper sense, consists of both the dilating and contracting efforts, as they succeed each other alternately.

For our belief that the heart is not passive in its dilatations, not a mere remission from action, but as much the effect of active powers of the heart, as its contraction, we quoted the experiments of Bichat. It is quite amusing to hear Dr. M's. objections to the evidence of this great physiologist. "If the reviewer," he says, "had given one sober thought to Bichat's article on the phenomenon of relaxation in organic muscles, he would have perceived that that great man was led, by his passion for analysis, in direct opposition to *reason* and *fact;*—that each, and every one of the six reasons advanced in proof of his *idle* doctrine, is alike destitute of foundation; and finally, that the whole error has arisen from a *careless* performance of a very common experiment." We will extract the six arguments of Bichat on this head: 1*st*, "When we lay bare a hollow muscle, the heart, the stomach, or the intestines, and empty it entirely of the substances that it contains, it contracts and dilates alternately, as when it is full, if we apply an external stimulant to it. 2*d*, If we empty by punctures, all the great vessels which go to the heart, or come from it, so as to evacuate it entirely, its alternate dilatations and contractions continue for some time. 3*d*, In order to judge comparatively of the degree of force of the contraction and dilatation, we can extract two hearts, nearly equal in size, from two living animals; place immediately the fingers of one hand into the auricles of the ventricles of the first, and grasp with the other hand the exterior of the second; you will feel that one makes as great an effort in dilating, as the other does in contracting. This fact already observed by Pechlin, is so *much the more remarkable, as the effort of dilatation is often greater than that of contraction.* I have even observed in *repeating this experiment*, that whatever effort we make with the hand, we cannot prevent the organ dilating. 4*th*, The alternate extension and contraction from which the vermicular motion of the intestines arises, is seen

during hunger, when we open the abdomen. 5*th*, The hardness of the organic muscular texture, is as evident during dilatation, as during vacuity. 6*th*, I have many times observed at the instant in which I irritated the heart with the point of a scalpel, that dilatation was the *first* effect of it, and that contraction was only consequent to it."

Let us now attend to the observations of our theorist, on the foregoing observations of Bichat. "It is most astonishing," he says, "that so acute a physiologist could have overlooked the real state of this case, (*i. e.* the third experiment on the heart,)—*that he should have mistaken for the diastole in the last mentioned heart, what was in reality the systole.*" A very modest declaration truly! Bichat tells us that he often repeated the experiment, and that he frequently found "the effort of the dilatation greater than that of contraction;" he must therefore have compared the dilatation with the contraction, and consequently distinguished them from each other. If he mistook the contraction for the dilatation, still, the fact of an active dilatation is substantiated, as the degree of effort experienced in either state was nearly equal. Again Bichat says, that "in irritating the heart with the point of a scalpel, he has often seen that dilatation was the *first* effect, and that contraction was only consequent to it." Does Dr. M. pretend to say that Bichat could not tell the difference between the dilatation and contraction of the heart when it lay before him? The observation of Dr. M., that Bichat was led by his *passion for analysis*, in direct *opposition to reason and fact*, is novel, and may be prodigiously wise, although to us it sounds very much like nonsense. How a passion for analysis should lead a man in opposition to *reason* and *fact*, we must leave to Dr. M's. "penetrative acumen," to determine.

In our review we say, "The principle of contraction is elastic power, &c. &c. This *elasticity is what Bichat calls the contractility of texture.*" Here our sapient theorist exclaims with great surprise: "What Bichat calls

contractility of texture!!" and then exultingly quotes from Bichat, as he imagines, to our utter discomfiture. The passage in our review is as follows: "It must be recollected that the arteries are not muscular tubes; that they do not possess the irritability and contractility peculiar to muscular structure; that, in short, *their principle of contraction is totally distinct from that of the heart; it being the consequence,* not of an excitation of muscular contractility, *but of an elastic power inherent in the arterial structure,* by virtue of which these vessels dilate or contract, according as they are distended by mechanical *force. This elasticity is what Bichat calls the contractility of texture.*" Now let us see whether Bichat will bear us out in this assertion.

Bichat divides the properties of the arterial tubes into two classes. 1st. Physical properties; 2d. Properties of texture. *Physical elasticity* constitutes the first class: and contractility of texture belongs to the second. These two properties are essentially distinct. Physical elasticity has nothing to do with the *contraction* of the arteries. "This elasticity, says Bichat, keeps the parietes *apart,* though they may be *empty.* It is to this elasticity of the arterial parietes that must be referred their recovering themselves when they have been flattened so as to obliterate the cavity. This elasticity requires for its exercise a previous *compression,* and is manifested by the sudden return of the parts to their natural state. Elasticity is purely a physical property." Of the contractility of texture, he says, "The contractility of texture takes place only when there is want of distention of the arterial parietes; that is, when the vessels cease to contain the blood which resists their contraction, or when they are *cut and afterwards left* to themselves. Contractility of texture has a permanent tendency to contraction; it is a property of vital matter *only.*" Hence the contraction of the arteries, according to Bichat, depends solely on *contractility of texture,* and on the other hand, their power of recovering their diameter, when mechanically compressed, on their physical elasticity.

When, therefore, we say, as we do in the above paragraph, that "the principle of contraction (in the arteries,) is totally distinct from that of the heart, it being in consequence of an elastic power, *inherent* in the *arterial* structure, and which power Bichat calls the contractility of texture;" we quote Bichat correctly, and apply his term, according to its correct signification. The idea which we wished to express, and which we obviously did express, in the above paragraph from our review, is, that the principle of contraction in the arterial tubes is elasticity; which elasticity is *inherent* in the *arterial structure*, being the same power which Bichat calls the contractility of texture.

Dr. M. says, that when an artery is cut off, the "ring" is neither larger nor smaller than the *elastic coat* makes it. Let him read Parry's and Bichat's works attentively, and he will find, that what the former calls *tonicity*, and the latter *contractility of texture*, keeps the end of a cut artery, while alive, in a state *considerably more contracted* than it is when these powers cease, in consequence of the death of those animals, and when *physical elasticity* alone is in force; and that, consequently, it is incorrect to say, that "the ring is neither larger nor smaller than the *elastic* coat makes it."

In our review we stated, in conformity with the views of Bichat, which we believe to be correct, that the capillaries *dilate* and contract altogether independent of the heart and arteries. Against this position Dr. M. observes: "A circular fibre can only shorten and not lengthen the diameter of a vessel;" and hence a capillary vessel can have no power of dilatation in itself, but must depend for the degree of its dilatation, on the quantity of blood which the heart forces into it; "What then," he says, "becomes of the reviewer's surpassing assurance, that the dilatation of the capillaries is altogether independent of the heart?" If the want of proper knowledge, which this interrogation betrays in the doctor, can

put a *blush* in his countenance, he will bear a demonstration in his face, that the capillaries can and do dilate "altogether independent of the heart." We shall be glad to know from the doctor how the heart influences, or rather, determines the sudden dilatation of, and flow of blood to, the capillaries of the face in blushing. The following facts go to demonstrate that the doctor's dictum, in relation to elongation of a circular fibre, is not correct. "When we lay bare a hollow muscle, says Bichat, the heart, the stomach, or the intestines, and empty it entirely of the substances that it contains, it *contracts* and *dilates* alternately if we apply an external stimulant to it." What now becomes of the doctor's "surpassing assurance," in saying, that "a circular fibre can only contract, and not lengthen its diameter?" Again, a muscle laid bare, and extracted from the body, contracts, and afterwards *dilates*, without being drawn to its state of dilatation by any cause. In an amputation, we often see in the stump the end of the divided fibres alternately lengthen and contract. The iris too is an example of this kind of dilatation.

Dr. M's. assertion, that the theory of Bichat, in relation to the change in the capillaries which determines dilatation in them, is "reduced to the true doctrine of dilatation from *debility*, actual or relative," betrays an ignorance of the true import of Bichat's doctrine. By organic sensibility every organized vessel admits or rejects fluids according as it is able or not to support them. Thus, the lacteals absorb chyle only, because the qualities of this fluid are in accordance with the kind of sensibility with which these vessels are endowed; but they refuse other fluids, because such fluids are not in accordance with their sensibility. This specific, or organic sensibility of a capillary is capable of undergoing modifications, and the capacity of the vessel for admitting fluids is then also modified. Such a change, however, does not consist in *debility*. The vessel may be as strong after the change in its organic sensibility, as before it took place; it has only

acquired a new modification of sensibility which disposes it to admit either fluids, that it was not previously able to receive, or larger portions of that which it naturally conveys. The pylorus, when the food is first received into the stomach, contracts, because the food in that state is not in accordance with its organic sensibility. When the food has become digested, its new properties put it in a proper relation with the specific sensibility of the pylorus. This passage then opens and suffers the digested mass to pass through it. It would be absurd, however, to call this change, *debility*. If the food remain long in the stomach, without undergoing a proper change from the digestive process, it changes the organic sensibility of the pylorus, which then admits it in an imperfectly digested state. It is, therefore, not true, that a " change of organic sensibility, by which dilatation is produced, is reduced to the true doctrine of dilatation from debility."

Our theorist thinks it " surpassing assurance" in us, to say, that " the dilatation and contraction of the capillary vessels, depend on a principle altogether different from that which determines the same changes in the arterial tubes." It is pleasant enough, to hear the author of this splendid theory we are considering—call it " surpassing assurance," to maintain an opinion which Bordeu, Bichat, and other able physiologists, have considered as true. The doctor, however, is exerting himself to say a great many smart and severe things, and for which he is certainly to be excused, as it was very unpardonable in us to show that his theory, instead of being " the waters of Jordan," is in fact but a stagnant and very shallow pool. Dr. M. cannot help but smile at our " solemn annunciation, that the capillaries, which *act* by an inherent principle of contractility, are in every inflammation dilated, and, if you please, debilitated, while in fever, on the contrary, the arteries are enlarged, not by their own action, but by the larger quantity of blood forced into them by the heart." But, says the smiling theorist, " if dilatation follows weakness, why does it follow it; because, as before said, there is forcible

injection by the action of the heart." In the first place we may observe, that we do not admit, that the capillaries *are* debilitated in an inflamed part; our expression " and if you please debilitated," was merely conceding a point, for argument's sake, and which we did not intend as declarative of our own belief. We believe, with Hunter, and others, that the capillaries enlarge by " an act of dilatation," the opinion of Dr. M. to the contrary notwithstanding. But we must stop replying to the quibbling of our theorist:—We will only add a few more remarks on the absurdities into which his theory leads.

In the first place we are told, that the heart and arteries, act most violently in the cold stage of fever; and that they act with less violence in the hot stage; because, in the former the pulse is contracted, in the latter, it is expanded. Now, according to this idea, we must admit the absurdities, that the more vigorously the heart acts, the more feeble is the circulation; and on the contrary, the more feebly it acts, the greater is the velocity and momentum of the blood. We must also admit, that cold, bleeding, and syncope, excite the heart to more vigorous action, while, brandy, camphor, and opium, render its action more feeble. Now, although, we must admit all these, and a great many other ridiculous conclusions, if we adopt this theory; yet the author seems wroth, at our refusing to consider it as very rational and excellent. Dr. M. concludes by advising us to study the physiologists; this advice is certainly good, and we do not object to adopt it; as we would not, however, be outdone in offering friendly counsel, we beg leave to recommend to him, a very careful study of Reids' work on the principles of Common Sense.

E.

ANALECTA.

1. *Cholera Morbus of India.*—M. Moreau de Jonnes, who has been occupied during the last four or five years, in investigating the nature of this disease, makes the following remarks respecting it: That it has traversed an extent of country 1340 leagues North and South, and 1900 from East to West. *2d*, That it is not governed in its attack by individual predisposition; by age, sex, or temperament. *3d*, That it does not depend upon the state of the atmosphere, but is equally severe at all seasons. *4th*, It is not the effect of humidity, nor confined to low marshy situations, as it appears in the mountainous region of Nepaul, the highest spots in the isle of France, in the sands of Arabia, and the desarts of Diarbekir; consequently, it owes its origin to some other source than marsh miasmata. *5th*, Its long continuance (seven years) proves that it cannot depend upon a vitiated state of the air. 6th, That it is not consequent to peculiar diet, as the rice of Oude, or the fish of the Ganges, as many have fallen victims to it who have not used these, or similar articles of food. *7th*, It is not transported by currents of air, as it has been seen to spread in a direction contrary to that of the prevailing winds. *8th*, That this disease has no connexion with common cholera, but is a pestilential and contagious malady of a peculiar character.*

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2. *Hydrocephalus.*—Dr. Maxwell, of Dumfries, has made some observations on hydrocephalus, illustrative of the good effects of blood-letting. The following instance describes a practice which has been successful in a large number of cases. The patient was bled in the jugular vein, "till syncopy began to take place; a little negus was then given; when the pulse revived, the finger was removed from the orifice and the blood allowed to flow until the pulse could no longer be felt. The patient had at this time no appearance of life, and continued without the least symptom of animation for ten minutes, when he began to revive gradually, and in the evening more favourable symptoms appeared. His mind was remarkably improved, as well as his physical powers, being now able to articulate, although indistinctly, and to tell the hour on a watch. He had a tolerable night's rest, having taken frequently some water-gruel and beef-tea. During the following day there was but little improvement, on the third day the bleeding was repeated in the left jugular vein, and a complete recovery followed."

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3. *Opium in Inflammatory Diseases.*—Dr. John Armstrong, author of a late Essay on Typhus, has made some valuable observations on the use of opium in inflammatory diseases. He ascribes the neglect of this remedy, in a great measure, to a general belief that it is merely a stimulant.

In peritonitis he has used it with great success, in combination with calomel, premising its exhibition by copious blood-letting. He gives it in doses of from two to four grains, with about as much calomel, and if this produces no good effect in the course of five or six hours, it should be repeated. He observes that he has seldom found it necessary to use more than two bleedings, and two doses of calomel and opium in any case; a third process, however, is to be gone through after an interval of six hours from the second, if necessary.

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4. *Hydrogen.*—A curious fact in relation to hydrogen gas, has lately been observed. If a stream of this gas is directed upon powdered platinum, it is inflamed. To insure the success of this experiment, the platinum should be slightly heated,—perhaps to divest it of accidental moisture.

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* We will give some observations on this interesting subject in our next number.—*Ed.*

5. *Experiments on Variolous Inoculation.* By WILLIAM MAXWELL, M.D., Dumfries.—In 1801, the matter of small-pox was kept for a fortnight and inserted; the disease exhibited the common symptoms of variola. "Matter taken from this subject was used at the end of a month, without any deviation from the usual symptoms. Inoculation was again repeated at the end of six weeks, as in the foregoing experiment, without any sensible alteration in the result. In experiment fourth, matter was taken from the last case and kept two months before insertion; when the patients, two in number, appeared to suffer less from fever; the pustules were earlier than usual; I thought they scabbed sooner, and had a narrower inflamed margin; in short, that the disease began to acquire a sensible degree of mildness. In experiment fifth, at the end of ten weeks, matter taken from one of the subjects in last experiment was used in several children; in some of them it failed to produce infection; where it succeeded, the disease appeared less active: the pustules began to scab on the sixth day. Experiment sixth was made with matter taken from one of the subjects of the preceding one; it was inserted at the end of three months; the eruption appeared on the fifth day with little or no fever; the scabs fell off sooner than in the last experiment, but in this there might have been some inaccuracy, as I had it only from report. Experiment seventh was made at the end of fourteen months; two subjects only out of nine who were inoculated, were infected; the eruption appearing on the end of the fourth or beginning of the fifth day, without fever or indisposition: the pustules were smaller, more prominent, like the small end of an egg, and almost entirely free from inflamed margin: the scab, of a foliated structure, fell off about the eighth day, without leaving any pittings. Four sets of children were now inoculated with this matter, applying it directly from the pustules: the disease in all respects resembled that in last experiment, the children playing about in a state of perfect health.

Although the periods of inoculation are said to have been at the ends of weeks or months, they were sometimes a few days sooner or later, but not varying more than four. This irregularity was occasioned by attention to my own convenience, as most of the experiments were done in or near the village of Newabbey, a distance of seven miles from Dumfries. I have not heard that any of the subjects of the above inoculation have been infected by either small pox or chicken-pox, although they have both been in the village, and no means were taken to prevent communication with the infected. Here the author's investigation ceased, giving way to vaccination, which extended rapidly."

These experiments go far to show that the varioloid disease and small-pox, if any doubt could now remain on the subject, are one disease. If it be true, as proved by the experiments of Dr. Carpenter some years ago in Philadelphia, that the vaccine pustules can be produced by the inoculation of the cow with the small-pox, we have the whole enigma solved of the success of the Jennerian discovery. The different degrees of power of the variolous contagion will explain the existence of chicken-pox, of the varioloid disease and variola, from the agency of one poison alone: Future observations must determine how far, like measles, to which some of the varieties of the varioloid disease approximate, it may originate from causes probably connected with the state of the atmosphere.

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6. M. GRAEFE's *Case of Lithotomy.*—The operation was undertaken under very unfavourable circumstances in a countryman aged thirty-eight, who had suffered from calculous symptoms nearly thirty-five years, and was almost exhausted by pain and misery. That the stone was large could easily be seen beforehand, but after the bladder was opened its size was found to exceed all expectation. So great was it that the forceps, however carefully applied, slipped off, or sunk into the concretion, or even bent at the handles; but all in vain. Professor Graefe at last succeeded in extracting it, by in-

troducing three lithotomy scoops into the bladder, and using them (with the aid of an assistant) as levers. Immediately after its extraction, the stone weighed twenty-one ounces and nearly four drachms. Its length was four inches, three lines; its breadth three inches, nine lines; its thickness two inches, ten lines; its longest circumference eleven inches, nine lines; its shortest nine inches, ten lines—all Paris measure. The patient was at first much relieved, but unfavourable symptoms soon appeared, and he died on the 11th day. The section showed that the wound in the bladder had contracted to the size of two inches; that the bladder had not been lacerated, and that it was not even inflamed. From the immense size of the stone which had completely filled the bladder, and therefore constantly obstructed the passage of urine, the ureters were dilated as high up as the kidneys, into large bags which had contained the urine.—*Anderson's Quarterly Journal, July,* 1824.

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7. *Of the Section of the Fifth Pair of Nerves.*—M. Magendie has recently communicated to the Academy of Sciences the result of his researches on the functions of the fifth pair of nerves. He found, after dividing this pair of nerves, that several senses were suspended. Believing that this arose merely from the injury occasioned by the operation, it did not at first excite his attention; but remarking that the animals thus operated upon did not recover their senses, he directed his inquiry to these points. The same results attended his experiments on the sense of smelling: in the memoir presented to the Institute, he also states, that after the division of the fifth pair of nerves, the eyes are deprived of their sensibility, and of a considerable share of their appropriate functions—sight is nearly lost: they quickly become the seat of inflammation, and waste rapidly. The tongue also loses its sensibility; and sapid bodies seem to produce no sensation on the anterior part of the organ of taste.—*Lond. Med. Rep. July,* 1824.

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8. *Strychnine.*—In the nux vomica, the bean of St. Ignatius, and the celebrated poison of Java, there is a peculiar vegetable alkali termed strychnine, one of the most virulent poisons in nature, as we have before said. It is soluble in water slightly acidulated, and so intensely bitter, that it is decidedly perceptible when diluted in *sixty thousand* times its own weight of water. The sixteenth part of a grain paralyses small dogs in four or five minutes, and kills them in less than half an hour. Its action is peculiar and uniform, paralysing invariably the hinder extremities.—*Lond. Medico-Chirurg. Rev. June,* 1824.

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9. *Physiological Action of the Salts of Bark.*—A considerable number of experiments have now been made to determine the medical or physiological effects of these medicines, and it is well ascertained, that they possess the properties of the cinchonas, and consequently that they may, in almost all cases, be substituted for them. It is, in the first place, desirable to ascertain the precise dose of all active remedies, and this is done better with the alkalis in question, than with the bark, from which they are obtained, and in which they exist in different quantities according to the state of the bark. In the second place, it is often, we might say always, desirable to administer medicines in a small volume, and agreeable form. In these respects the salts possess great advantages. Chemistry, therefore, has done much service to medicine, by separating the active principles of vegetables from the inert substances with which they are combined in a natural state.

The sulphates of quinine and cinchonine may be given in doses varying from one to ten grains in the twenty-four hours. Some have carried the dose much higher, but this is seldom necessary. Large doses, indeed, have produced inconvenience, if not danger, giving rise to strong cerebral excitement. A wine or tincture of the sulphate of quinine may be made thus:

—Sulphate of quinine, 10 grains; Madeira wine, two pints. The tincture—sulphate of quinine, five grains; alcohol, one ounce.—*Lond. Medico-Chirurg. Rev. June*, 1824.

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10. *Effect of Colchicum.*—" Mrs. ———, aged forty, after frequently suffering from gout, requested her medical man to give her the colchicum in a very severe fit.

" She took ʒiiss. of a tincture made by infusing ʒiv. of the root in ʒviij. of proof spirit for three days, the mixture being kept at nearly 100° of temperature. This was given in the morning of Dec. 5. In the evening it had produced no effect, except slight qualmishness. Calomel gr. iij. opii gr. i. was ordered at bed-time, and a purging draught for the morning. However, in the night, vomiting and purging commenced, and continued all the next day, in spite of effervescing volatile saline draughts with opium; so that in the evening of the 6th, oppii gr. i. camphor gr. iii. were given and repeated in two hours.

" On the 7th, from accident, she was not seen till three *p. m.* when she was found in the collapse preceding death. The gout had previously gradually subsided. It was stated that she became faint at two o'clock *p. m.* and not till then were her friends alarmed. By opium and spirits, warmth was reinduced upon the extremities, and a feeling of greater comfort produced; but the pulse never completely recovered, although the sickness was completely subdued; so that at ten *p. m.* she fell into an *apoplectic kind of sleep*, which terminated in death before morning."—*Lond. Medico-Chirur. Rev. June*, 1824.

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11. *Intestinal Tympanites.*—Dr. Levrat, Physician of the Hotel Dieu of Lyons, has related a curious case of this kind which was cured by puncturing the intestine, and setting free its gaseous contents. The patient was Madam Lepin, to whom he was called on the twenty-fifth day after a painful delivery, in which the fourchette was torn. He found the patient apparently dying; with facies hippocratica, small creeping pulse, cold extremities, and tumefied abdomen, which was very painful on pressure. Cordials, anodynes, and emollients were first prescribed, and removed the most urgent symptoms; but intestinal tympanites remained, and continued in spite of various medicines to increase to an enormous extent. The convolusions of the bowels could be distinctly traced, and the air could be heard rushing from one knuckle of intestine to another. Emaciation had made great progress. Dr. L. determined on an operation. A very delicate trochar was constructed, not much larger than a darning needle, and about two inches in length. This was pushed through the parietes of the abdomen, on the right side, midway between the navel and the spine of the ilium, and into a projecting roll of inflated intestine. Immediately the stilet was removed, a quantity of gas rushed out through the canula, and the belly experienced a great reduction in size. The patient passed a good night, and next day a large quantity of hardened scybala came away, with infinite relief to the lady. From this time she went on well, and rapidly recovered. We forgot to mention that the canula (corked) was left in for some hours, and then some more gas drawn off.

This operation has been practised before, for tympanites, but not, we believe with success—probably because the wound in the parietes of the belly and in the intestine was made with too large an instrument, in consequence of which, gas or other matters may have escaped from the intestinal tube into the general cavity of the peritoneum, and there excited inflammation. In Dr. Levrat's operation, the instrument was nearly as fine as that used in acupuncture, and consequently the wound in the intestine was so small as not to permit the escape of any intestinal contents into the

cavity of the peritoneum, when the trochar was withdrawn.—*Bulletin des Sciencis.*

12. *Antimony in the Phlegmasiæ.*—A mystery and doubt still hang ovee the Italian mode of treating the phlegmasiæ by large doses of the tartratr of antimony. Dr. Fontaneilles has recently published his testimony respecting this litigated question, in the ARCHIVES GENERALES for February last, from which we shall abridge some particulars. We need hardly remark that the employment of tartar emetic is no new practice in the phlegmasiæ—but it was only considered as an auxiliary to blood-letting and other evacuations. The peculiarities of the Italian practice, and especially that of Rassori, consist, 1mo. in treating pneumonia, from beginning to end, with this medicine—2do. in making it the principal, or almost the sole remedy—3tio. to thus reduce, or rather discard blood-letting.—4to. to exhibit the medicine in doses that would appear frightful to former practitioners—that is, to the amount of a scruple, or even some drachms in the twenty-four hours, and that without producing either vomiting or violent purgation.

The first accounts of this practice staggered the belief of physicians, who insisted that some fallacy must have crept into the reports. It was said that the medicine could not have been of its proper strength, or that the patients could not have swallowed the doses that were prescribed. Both these doubts have been cleared away, if we can credit the assertions of Dr. Fontaneilles (Physician of the Military Hospital of Milan) who has given the medicine to be analysed and experimented on by those who were competent to the task of both. Many physicians scrutinized the exhibition of the remedy in the doses described—without being able to detect any fallacy in the practice.

Our author observes that the capability of sustaining such large doses of emetic tartar depends entirely on a certain morbid state of the system—for when this morbid condition is removed, the power of taking the remedy in large doses with impunity or advantage immediately ceases also. This, he insists, is the true and only solution of the enigma; and this indeed is most indubitably the cause, as we have hinted in a former number of this journal. This morbid condition, or diathesis, which bears the reception of such large doses of the antimony, varies in different cases of peripneumony, (for it is of peripneumony which our author treats in this paper) and in different stages of the same case. Thus, this diathesis is less prominent in the beginning and in the decline of the inflammation—and most powerful at the acmé of the disease. The doses of the medicine must be regulated according to these epochs, otherwise their common effects will be produced. In general our author commences with not less than twelve grains in the day, and the same quantity in the course of the night. But when he finds that the pneumonia has made progress, he gives from a scruple to half a drachm in each of the above periods, gradually augmenting the quantity, according to the violence of the disease. Sometimes, but merely as exceptions to general rules, the dose of the medicine exceeds the ratio of the diathesis, and then vomiting is the result. At other times, the violence of the symptoms, as the cough, pain, dyspnœa, &c. will abate, while the force of the diathesis remains undiminished—as known by the capability of bearing the large doses still continuing. We must not therefore diminish the dose with the diminution of the symptoms—*unless we find the power of bearing the medicine also diminish.* Again, some of the troublesome symptoms may persist, notwithstanding that the diathesis is declining. "Thus the breathing may become more short or laborious, or delirium and tendency to coma may supervene—and, if, in these cases, we find a diminished aptitude in the constitution to bear the large doses of antimony, we may conclude that organic changes are taking place, which will set at nought the resources of our art."

The cessation of the diathesis is soon proved by the patient not being able to bear the smallest quantity of the tartar emetic, without loathing and disgust, although previously swallowing large quantities without the smallest inconvenience, but, on the contrary, benefit. Although our author looks upon emetic tartar as a direct sedative, and in this respect similar to digitalis, yet he observes that its exhibition, in full doses, is not attended with the same bad effects as sometimes attend the latter medicine. He has never seen the pulse reduced below fifty in the minute by antimony, and but rarely any irregularity in the action of the heart.

Our author has been asked why he does not abandon blood-letting altogether, since he has found so effectual a remedy for pneumonia in emetic tartar? His answer to this question is, we think, a very wise one. In many cases, says he, the pneumonia runs its course so rapidly, that the structure of the lungs is menaced with destruction, which circumstance induces to put in force, *simultaneously*, different means of reducing the phlogistic diathesis. On the other hand, he deprecates the subtraction of so many pounds of blood in this disease, when venesection may be so powerfully aided and greatly abridged by the use of antimony. He properly impresses it on practitioners that blood-letting is never to be dispensed with in this class of maladies, especially when they are severe, since the effects of this remedy are prompt and certain, where time is so precious; whereas, it requires a certain period to elapse before the antimony produces its operation in checking the inflammatory process.—*Med. Chirur. Rev. June*, 1824.

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13. *Belladonna as a preventive.*—Among the very few novelties in the department of Therapeutics we have to introduce to our readers, is the system of Dr. HAHNEMANN, under the name of *homœopathia*, which is used to designate the method of treating diseases by means of active medicines in doses of extreme minuteness. The fundamental principle of this theory is, that different complaints are cured by means which produce symptoms analogous to those of the disease: and that the remedy is appropriate in proportion to the strictness of the analogy between its effects and the disease it is intended to cure. Thus, a chronic disease may be arrested by an acute one, and vomiting stopped by an emetic: in short, the principle is *similia similibus curantur*. Dr. Hahnemann, proceeding upon this idea, argues that, as an atom of vaccine virus is sufficient to give protection against small-pox,—or an atom of any other morbid poison, as syphilis, is capable of giving rise to a disease similar to that in which itself has originated,—so atomic doses of active medicines, provided they be the proper contra-stimulus to the disease, are regarded as preferable to the same medicines in larger quantities. There appears much of hypothetical assumption in these doctrines, so far as we have yet been made acquainted with them; but numerous plausible cases are cited,* and we are promised further details in a system of Materia Medica, by Hahnemann.

We have alluded to the views of M. Hahnemann, however, chiefly for the purpose of introducing one of the most important illustrations of his doctrines,—one which, rejected in France, and almost unknown in this country, is nevertheless regarded by many in Germany as second in importance to the discovery of vaccination alone: we allude to the alledged efficacy of *belladonna as a preventive of scarlatina.* We are told that the author of this system, having observed that the drug in question, when taken in very small doses, produced symptoms analogous to those of scarlatina, was hence led to the conclusion that it was the proper antidote to the disease; a conjecture which at first was regarded, even in Germany, as too improbable to merit serious attention. But, lest our readers should still think thus

* By Dr. WIEDMANN in HUFELAND'S *Journal der Practisch Heilkunde*, 11 cahier, 1823.

of Dr. Hahnemann, and his claims to be regarded as a second Jenner, we shall lay before them the opinion of Professor KOREFF, in a letter to M. LAENNEC; an opinion which, he informs us, is founded on the observations of sixteen years.*

"Observation clearly proves," says he, "that the belladonna, taken for some time, either in powder or in extract, produces, especially in infants, a redness of the skin, which is sometimes transient, but at others more durable; dryness of the mouth, with a sense of heat in the throat; dilatation of the pupil; anxiety; and occasionally swelling of the submaxillary glands: symptoms having a great resemblance to those which accompany the eruption of scarlatina. The effect of the belladonna has also this in common with scarlatina, that neither of them produce the redness of the skin invariably, whilst the symptoms about the throat are always present. I confess to you, however, that all these analogies did not appear to me sufficiently strong to persuade me that in this plant was really to be found a preservative against scarlatina, similar to that which the cow-pock affords against variola. It was not till I had received the authority of the celebrated Soemmering, who informed me that he had obtained the most satisfactory results with it when the disease raged epidemically, that I determined to employ it. This malady, accompanied by the most unfavourable symptoms, and having entirely changed its usual character, was at that time producing ravages almost as fatal as the contagious typhus. I then, for the first time, had the happiness to protect from this dreadful contagion almost all those who took the belladonna with a little perseverance, and of these there were many thousands. Since that time I have never lost sight of this discovery, which becomes the more valuable as the scarlatina has increased during the last thirty years, both in violence and extent, in many countries; and I have always found the same effects in different climates, and in epidemics of opposite characters. Many other physicians have equally confirmed the preventive powers of this plant, and the German Journals are daily filled with proofs of a benefit which, with respect to some countries, equals that of vaccination. In France, the capital and the provinces of which appear less subject to these fatal epidemics than Germany, Swisserland, the Tyrol, Poland, and the north in general, less attention has been given to this discovery, and it has been rejected,—it must be said too lightly, and without any sufficient examination, as may be seen in the article *Belladonna* in the Dictionnaire des Sciences Medicales. I only remember a single observation on this important subject, by Dr. MEGLIN,† who gives an account of a trial which he gave to this preservative during an epidemic of scarlatina at Colmar, and which confirms all the assertions of the German physicians. The absence of present danger is, perhaps, the cause of this indifference towards a discovery, which, important in itself, might also be fruitful in results applicable to other diseases. At present, however, I shall confine myself to an account of the results which have been ascertained, (by repeated observations, and by a great number of individuals placed in very different circumstances,) without incurring the reproach of having proceeded in a manner not sufficiently rigorous.

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"The powder mixed with sugar, or the extract made very carefully from the juice of the recent plant, are employed after the following formulæ:—Extract of belladonna, three grains, dissolved in an ounce of cinnamon water. Powder, or root of belladonna, two grains, mixed with ten drachms of white sugar, divided into sixty doses. From half a dose to a whole one is

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* *Sur l' Emploi de la Belladonne contre la Contagion de la Scarlatine: Note communiquee a* M. le Prof. LAENNEC, *par le* Prof. KOREFF.—(Bulletin des Sciences Medicales, Avril, 1824.)

† *Nouveau Journal de Medicine*, 1821.

given to a child from six months to two years old, four times a-day; to children from three to six years old, from a dose to one and a half; to those from six to nine, two to two and a half; to those from ten to twelve, three to four and a half. Of the solution, a drop is given for every year of the child's age, once a-day, and fasting. Observation has shown that, when the epidemic is very fatal, or the intercourse with the patients very frequent and intimate, it is prudent to increase the dose a little. It has not yet been possible to determine, in a satisfactory manner, the length of time which is necessary to eradicate, by this remedy, the susceptibility of the contagion. Every thing leads us to believe that the remedy, if used during a time too short to ward off the contagion, moderates very much the malignity of the disease. We know for certain that the remedy does not permanently overcome the disposition to scarlatina; and it is necessary to resume its use on every recurrence of an epidemic. We have always observed that the most intimate communication with the sick does not produce the disease, provided the medicine has been employed eight or nine times previous to being exposed to the contagion, and continued up to the period of desquamation; a circumstance very important to nurses. It appears more certain to begin with rather strong doses, in order to guard against the first impression of the contagion, and to diminish the quantity after a few days. No sensible effect has been observed to follow the continued use of this small quantity of belladonna. Up to the present time, neither season nor locality, nor any other circumstance, has appeared to diminish the preservative effect of this plant."

After remarking that the belladonna does not exercise the same power over miliary scarlatina, he proceeds:—"Do not believe, my learned colleague, that these results have been too lightly deduced, or from a small number of individuals, or from epidemics of little violence. It is from entire provinces,—from cities affected with this terrible scourge,—from epidemics the most fatal, in all seasons, and in localities the most diversified,—on individuals of every age and of every condition, that observations have been made with the greatest accuracy, and have led to the above results.

Belladonna has been long used by CHAUSSIER in cases of rigidity of the os uteri, in the following manner:—Rather more than two drachms (*huit grammes*) of the prepared extract are softened with an equal quantity of distilled water, and incorporated by trituration with an ounce of hog's-lard or simple cerate. In order to secure all the benefit which this means is capable of affording, it is necessary that this preparation be applied directly to the orifice of the uterus. To effect this, he has contrived a small syringe, which in place of having a pipe, is rounded at its extremity, and has an opening large enough to admit the point of the little finger. The piston is pulled back a little, then about the size of a small nut of the ointment is introduced at the upper orifice; the syringe is directed by the finger to the mouth of the uterus, the piston is pushed home, and the ointment forced out; being of a soft consistence, it liquifies and spreads over the parts. At the end of thirty, or at most forty, minutes, the os uteri becomes soft and relaxed to such an extent, as no longer to present any resistance to the efforts to dilate it, or to the contractions of the uterus itself. This method was extensively employed by the late Madame LACHAPELLE, and is likewise commended by M. LEGRAND, the present midwife in chief at the Maison d'Accouchement.*—*Lond. Med. and Phys. Journal, July*, 1824.

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14. *Colchicum Antumnale in Gout.*—Within a few years this remedy for gout obtained great reputation, and, indeed, was spoken of as new remedies generally are, in the most enthusiastic terms. Dr. Hall, of Philadelphia, used it in one case, in which the disease was translated from the foot

* *Journal Universal des Sciences Medicales*, 96 cahier.

to the brain, and it was only by means of excessive blood-letting, cathartics, and enemata, that he was able, in the course of forty-eight hours, to rouse the patient from the apoplectic state which threatened his life. As this metastasis was attributed to the colchicum, no more of this substance was prescribed, but a perfect cure was subsequently effected, by confining the patient generally to animal food, prohibiting all acid and fermented drinks, and using active exercise, and cold bathing. In addition to this treatment, a few cathartics and bleedings occasionally were necessary. We are induced to mention this case, as we find that the London Medico-Chirurgical Review contains the details of another, in which also the colchicum autumnale appears to have produced a determination to the brain. Dr. Hall has also witnessed a case, in the state of Georgia, in which a single dose of the vinous tincture of colchicum produced a dizziness and stupor, which, had it not been removed by immediate venesection, would, very probably, have run on to perfect apoplexy.

These cases indicate, that not only colchicum, but all other agents capable of arresting, suddenly, a paroxysm of gout, should, if used at all, be applied with great caution, and never in circumstances where the physician is not immediately within call of the patient.

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15. *Distortion of the Spine.*—Dr. Macartney, of Dublin, in a paper lately published in the Lon. Med. and Phys. Journal, observes, that distortion of the spinal column may arise from three causes. The first is produced by an ulceration of the bodies of the vertebræ, which induces an abrupt or sudden curvature. The second originates in a deficiency of earthy matter in the bones, and is curved into flexures, instead of being angular like the former. The third cause of distorted spine, which he states, is *a feebleness of those muscles which are employed in maintaining the erect position of the body.* The tendency of a weakened state of the muscles to augment the more serious kinds of curvature, and to induce, even without a softened condition of the bones, an evident distortion of the back, does not appear to him to have received sufficient consideration from medical men. Conformably with this view, he thinks that all confinement of the trunk of the body is not only calculated to produce the disease at first, by fatiguing and debilitating the muscles, but in a more especial manner, to confirm it after it has actually commenced. When it becomes necessary to relieve the spine from the weight of the superior parts of the body and the head, until by proper medical treatment the strength be sufficiently restored, he prefers the mode of resting on a flat surface, or, what is more comfortable, a gently-inclined plane, to any species of machine or bandage that has been invented. He thinks, however, that the effects of the recumbent posture are not always understood, and are sometimes over-rated. There appears to be no necessity for enjoining rest, as is usually done; on the contrary, the patient should rather be encouraged to exercise the spinal muscles by rolling about, and moving the arms and legs at pleasure: for which purpose, a large platform, stuffed with wool, or a matrass, or where children can have their toys, and play with their companions, is preferable to a sofa or plane board, which are most frequently employed.

The recumbent position is sometimes resorted to as the sole means of curing or preventing curved spine. In several celebrated boarding schools in London, it is used indiscriminately for all species and degrees of distortion: and, in some of the seminaries, all the girls are condemned to get their lessons during so many hours every day, in the surpine posture, with the view of preventing the occurrence of deformity. It appears to him more than unnecessary to confine healthy girls to the recumbent position: yet too much caution cannot be used to prevent children leaning uniformly to one side, in the act of writing, drawing, &c., more especially if they be weakly, or if they seem to prefer that particular position.

The advantages from exercising the spinal muscles in various positions of the body, should never be lost sight of; both as the means of preventing and of curing curvature, when the erect position can be safely sustained. In some cases, where the curvature has been slight, the persons very young, and the general health good, he has succeeded in restoring the natural form, merely by liberating the body from all constraint of stays or monitors, and permitting as much exercise to be taken as could be borne without fatigue.

Balancing any object, however light, on the head, has great efficacy in rectifying the shape of the spine. It is also readily adopted, as a pastime, by young people. In some instances of spinal curvature, where there was not a softened state of the bone, he has tried, with the best effects, exerting the muscles of the back, by the carriage of a bag of sand on the head; employing, at first, short periods of time for the exercise, and allowing rest in the intervals, in the recumbent posture; and, as the strength increased, repeating the exertion more frequently, and continuing it longer. He has every reason to believe that this treatment, if employed with proper caution, will be always useful in cases of curvature from weakness; and, if practised with assiduity during early youth, he is persuaded would be the best means of preventing distortion.

The means which are most beneficial, where merely the shoulders are round and prominent, are skippings with a rope, and the use of the dumb-bell; particularly in the action of carrying them backwards, which exerts the flat muscles that lie between the shoulder-blades and spine.

He concludes by observing, that in all cases where a curvature of the spine, from a softened state of the bones, is apprehended, or has taken place, every endeavour must be made to improve the health and strength, by a pure air, tonic medicines, and the cold bath; without which, machinery and rest will be useless, and exercise hurtful.

Some situations are much more productive of a disposition to curved spine than others. In certain districts of England and Holland, it is found to prevail. In the neighbourhood of Leyden, he has observed great deformity of the spine to be almost universal with the women, and to exist even frequently in men. Damp and low situations would seem to have a peculiar tendency to induce a softened state of the bones.

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13. The *pulvis antimonialis* of the London Pharmacopœia, was made the subject of experiment by Dr. Elliotson some time ago, with a view of ascertaining its real effects and proper doses. We are inclined to think his observations have not met with the notice they deserve,—at least, they have not had any influence with those to whom we are indebted for the New Pharmacopœia. Dr. F. Hawkins, apparently ignorant of Dr. E's. remarks upon this subject, has published a paper,* in which he has arrived at similar conclusions: indeed, he asserts that the effects attributed to this medicine, are not to be produced by ten times the usual dose; and he informs us, that he now begins by exhibiting half a drachm at once. One case is mentioned, in which two scruples were given twice a-day, for several weeks: "no function was in any degree affected by it." In another instance, so much as one drachm was exhibited night and morning, without any sensible effect. It is proper to remark, that Dr. H. procured some pulvis antimonialis from Apothecary's Hall, for the purpose of experiment.

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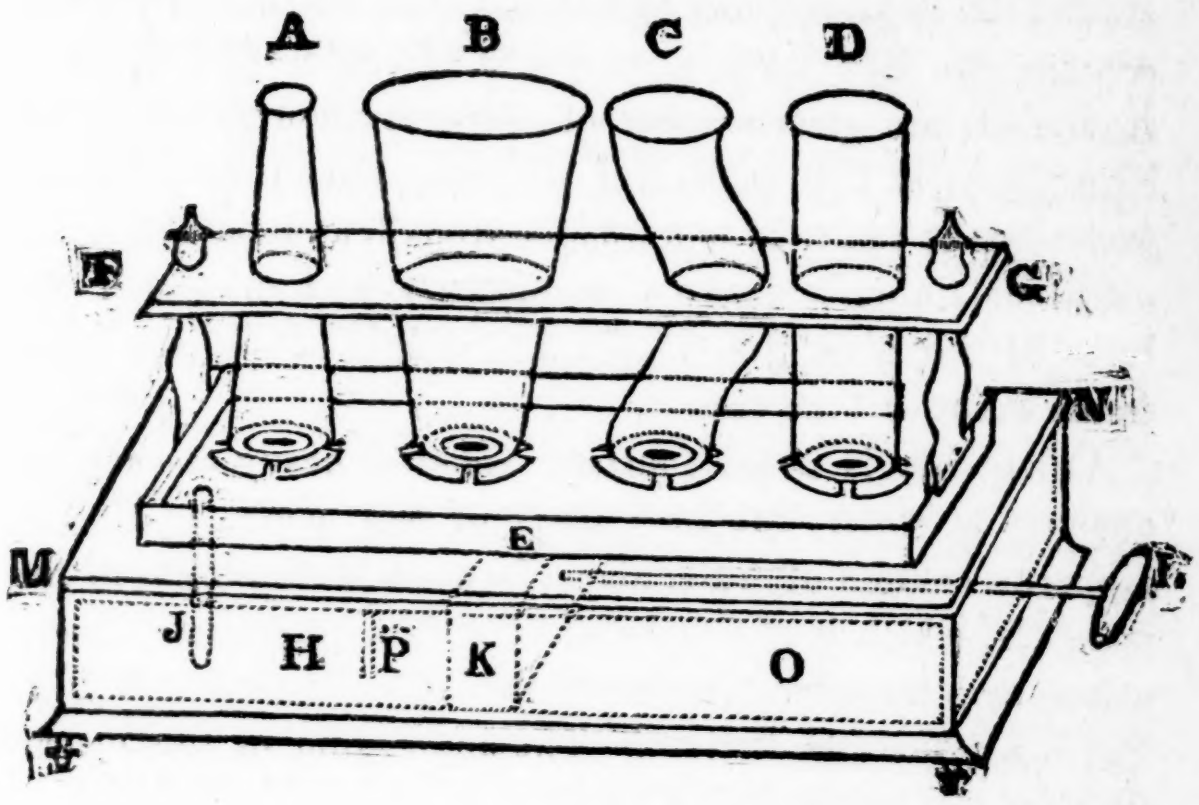
* *On the Dose of the Pulvis Antimonialis.* By F. Hawkins, M. D. &c.—(Edinb. Med. and Surg. Journal, April, 1824.)



A New Hydrostatical Instrument, by JACOB GREEN, Professor of Chemistry, &c.—A number of instruments have been contrived to illustrate the important principle in hydrostatics, that the pressure of fluids on the bottoms of the vessels which contain them, bears no proportion to the quantity of the fluid, but depends entirely on the shape of the vessel. At the first glance, it seems contradictory that the pressure of water, or any other fluid, on the base of a vessel shaped like a cone or an inverted funnel, should be so great as it would be, were that vessel equally wide from the bottom to the top, and which consequently would hold twice as much of the fluids.

The apparatus of Paschal, for the purpose of demonstrating the hydrostatical paradox, that a small quantity of a fluid may be made to counterbalance any quantity of the same fluid, is the instrument commonly in use; but I have always experienced great inconvenience with it before a class, from the sudden dashing out and splashing of the water from the stiffness of the valves, and other circumstances connected with the counterpoising weights. I propose, therefore, as a sub-

stitute, the following instrument, in which the vessels, which contain the fluid have a mercurial valve in their bases.— A B C D are glass vessels of different figures and capacities, open at both ends, and whose bases and altitudes are precisely the same: E is a wooden trough, an inch and a-half deep, containing a quantity of mercury, and in which the bases of the glass vessels are immersed. F G is a little frame to support the vessels.

As mercury is about fourteen times as heavy as an equal quantity of water, one inch deep of that metal in E will support a column of water about fourteen inches high, in the vessel D, which is of equal diameter throughout, and if more water than the above quantity should be poured into D, it will run out at the bottom. The application of this instrument to the hydrostatical principles which it is intended to illustrate, will readily be made by every one acquainted with the subject.

As the mercury used in this experiment would collect on its surface dust and other impurities, if suffered to remain in the trough E, there is a little cistern H, in the base of the instrument, where it may be preserved; into this cistern it is conveyed after experimenting, by the tube J, open at both ends, and which reaches from the bottom of the trough E nearly to the bottom of the cistern H. At the upper part of the side of the cistern marked P, there are two or three small holes bored, which form a communication from D to H.— K and L is a piston for forcing the mercury from H into E when required. The manner in which this is done will be easily understood by referring to the figure.

*** The Editors owe an apology to the Subscribers to this Journal, for the delay which has unavoidably been experienced in issuing the present number. As it proved impossible to complete it precisely at the termination of the last quarter, they thought it best to commence the year with the last half volume.